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Doctor George W. Wharton, Professor and Head of the Department of Zoology, University of Maryland, who extended a cordial and hearty welcome on behalf of the Host Institution.
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Scientists who attended the Fiftieth Anniversary Celebration, in front of the Student Union, University of Maryland.

Morning Meetings

PERSPECTIVES IN PARASITOLOGY

Introduction

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It is my pleasant duty to raise the curtain on the scientific portion of our program celebrating the 50th birthday of the Helminthological Society of Washington, an organization better known to most of us as "Helm Soc." Although we are an organization devoted to research in all branches of parasitology the name "Helm Soc" has endeared itself to us too much for us to think of changing the title to a more general one. The diversity of interests covered in our activities is well reflected in the scope of the papers and discussions planned for this momentous day in our history.

As I have said, my function here is to raise the curtain and I shall do that by saying a few words on the subject "Perspectives in Parasitology" which is the general title of our entire scientific program; not, as the printed program might lead you to believe, the title of a lecture to be delivered by me.

The word, *perspective,* has various meanings and undoubtedly brings different concepts to your minds. In my own mind it usually calls up a drawing of a railroad with the lines representing the rails and telegraph lines converging to a point on a distant horizon. I am sure that this is because my first contact with the word was in grade school drawing class. In a temporal sense this concept can be carried to a look into our past. We stand here today and look back toward that date, 1910, which is beyond the horizon for the great majority of you. Our program is planned to do this but only in one respect. We are not here to boast of our accomplishments of the past 50 years. However, in our informal program this afternoon we hope that by dividing into smaller groups we can recapture some of the spirit and enthusiasm of the small group which banded together here 50 years ago and started the Helm Soc. In some ways those were the halcyon days of science when individuals could assemble, speak a common language, and enjoy the give-and-take of round-table discussions.

The general emphasis we have tried to place on the scientific program today is characterized by another meaning of "perspectives." This is a more philosophical prospect in which subjects are considered in respect to their relative positions. At the time of the birth of our Society parasitology was still largely a special field of zoology. The study of parasites was either included in the course of invertebrate zoology, taught usually by someone who knew little about them, or because of the relation of some of them to disease of man or animals consideration was given to them in such courses as pathology. Although experimental methods had been introduced to unravel the many complex life cycles which were not yet understand, parasitology was predominantly a morphological and taxonomic subject. The vigorous growth of medical entomology was beginning to bring parasitologists into closer contact with other fields of microbiology such as bacteriology. Immunity to parasites was considered either not to exist in most instances, or to be governed by principles quite different from those known from the study of immunity to bacteria or viruses. Aside from the chemotherapeutic agents which had been discovered by accident or by trial and error, the science of chemotherapy of parasites had not really begun.

To shift from 1910 to 1960 in viewpoints and accomplishments is like shifting from the age when horse-drawn vehicles were the predominant mode of travel to our modern age of jet airplanes. We have come far in both instances. Parasitology is a scientific discipline which speedily is enlarging its vistas and joining hands in manifold ways with other biological as well as with physical and chemical disciplines. Barring some unwelcome world catastrophe, our expectation should be that not only will these associations in the future accelerate the progress in solving our parasitological problems but that the availability of our methods and materials may help biochemists and physiologists to enlarge the scope and expedite the solutions of some of their problems. In spite of the great distance which separates those of us working at the organismic and cellular level from those working at the atomic, molecular and macro-molecular levels we hold as an ideal the closing of these large gaps. The same general outlook holds for other areas of parasitology in which progress has been slow or much ground yet needs to be covered. Therefore, at our mid-century pause for taking stock of our past and planning our future we are faced with new and more complex problems hardly dreamed of in 1910 but we also see a slowly growing synthesis emerging from disciplines which were once far apart but now are close enough together to bring about mutual enzymatic action. It seems reasonable to hope that in the next 50 years we shall see developments more spectacular in kind and in rate of accomplishment far beyond that which has occurred in the past half century. The Program Committee hopes that the guide lines we have laid down for your program will make possible a fruitful day of viewing these new perspectives.

Some Dietary Factors that Affect Ovarial Transmission of Symbiotes*

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The distinction between parasite and symbiote, being man-made and quite arbitrary, probably becomes negligible when we consider the physiology of microorganisms which dwell within the cells of their hosts. Since Dr. Trager's address, which follows, deals with the physiology of intracellular parasites, my remarks will be limited to one aspect of the physiology of intracellular symbiotes. It is expected that what we learn about one category of microorganisms may be applicable to the other.

Symbiotic yeasts and bacteria are not at all uncommon among insects. The microorganisms may be either extra-or intracellular, and are found in almost any conceivable location; but the most numerous cases are those with some connection with the digestive tract. There is no obvious correlation between presence of symbiotes and phylogenetic position of the dozen or so orders of insects which possess them. However, with one or two exceptions, the insect hosts feed on a specialized diet deficient in vitamins, amino acids, or other essential nutrients. Because of these two facts—anatomical association with the gut and inadequate diet—it has been presumed during the years since microtomy has been developed that the symbiotes perform some nutritive function. As a matter of fact, in most of those cases which have been

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experimentally analyzed, this has proved to be the case (for references, see Richards and Brooks, 1958).

One of the earliest described cases of symbiosis, that of the cockroach (Blochmann, 1887), happens to be paradoxical and most complicated. The cockroach is a relatively primitive insect and an omnivorous feeder. Therefore it does not fit the generalization above that symbiosis is limited to insects restricted to impoverished diets. The symbiotes, which are called *bacteroids* because of their resemblance to bacteria, bear no connection with the gut. Instead they are contained in specialized cells (mycetocytes) distributed throughout a precise region of the abdominal, visceral fat body. The mycetocytes undergo migrations in the female insect that carry the bacteroids to the ovaries; and the bacteroids migrate in the embryo to infect the mycetocytes. Thus the relationship is perpetuated transovarially and is always found in every individual of all species examined.

The bacteroids are never exposed to the external environment. Moreover, they are always located intracytoplasmicly except for the brief period after they leave the mycetocytes and before they penetrate the tunica propria of the ovarioles.

Through long and intimate association with the insect host, the bacteroids seem to have become an integral part of the host cell. There is considerable debate in the literature as to whether they—and other intracellular symbiotes —may not actually be mitochondria or other organelles. In spite of numerous attempts to culture the bacteroids *in vitro,* their identity remains unproven. Obtaining aposymbiotic insects is, of course, an alternate approach to analyzing the association.

The bacteroids have been remarkably resistant to elimination through treatment of the host by heat or antibiotics. Only treatments drastic enough to produce high mortality in the roaches have had detectable effect on the bacteroids in the fat body. Similarly, because of the intracytoplasmic location of the bacteroids, it is impossible to use conventional methods of surface-sterilization of eggs to obtain aposymbiotic offspring.

However, it has been found that lifelong feeding of 0.1% aureomycin will eliminate the bacteroids from the eggs during the maturing process in the ovaries (Brooks and Richards, 1955 a). It is thus possible to obtain F_1 and subsequent generations practically free of bacteroids and thereby to study the physiology of the aposymbiotic insects. These creatures are weak, light-colored, and subject to early death if treated as normal cockroaches. They respond in a gratifying manner to a diet enriched with 25% brewers' yeast or lyophilized whole liver; that is, survival is good and on certain diets the growth rate equals that of symbiotic insects. But the color of the cuticle of the aposymbiotic roaches always remains light and a large percentage of their eggs are inviable.

This again indicates an involvement of symbiotes with nutrition. Probably they do not merely synthesize B-vitamins, as the other insect symbiotes have been found to do. It is more likely that they also metabolize certain intermediates (Brooks, 1956 (1958); Henry and Block, 1960).

EXPERIMENTAL

Work was initiated a few years ago in our laboratory to determine the nutritive role of the bacteroids. A necessary prelude was the determination of survival, growth rate, and reproductive capacity of symbiotic roaches on a standardized, minimal diet of identified constituents. The most comprehensive study of cockroach nutrition in published form at the time was that

of Noland *et al* (1949). Their diet supported good growth but reproduction was poor. The few nymphs that hatched lay on their backs, kicking feebly, and died in a few days. The authors suggested that this phenomenon might be due to lack of a "reproductive factor" in the food. We examined our nymphs histologically and determined that they lacked *symbiotes*. In other words, this was a diet which was good for growth, but which either lacked an essential or contained an inhibitor; so that it had the same effect on reproduction as does aureomycin.

Fortunately, we also had a second diet formula in unpublished form devised by Gordon (and subsequently published in a slightly revised form, 1959) which was satisfactory for both growth and reproduction. A high concentration of bacteroids was transmitted through the eggs.

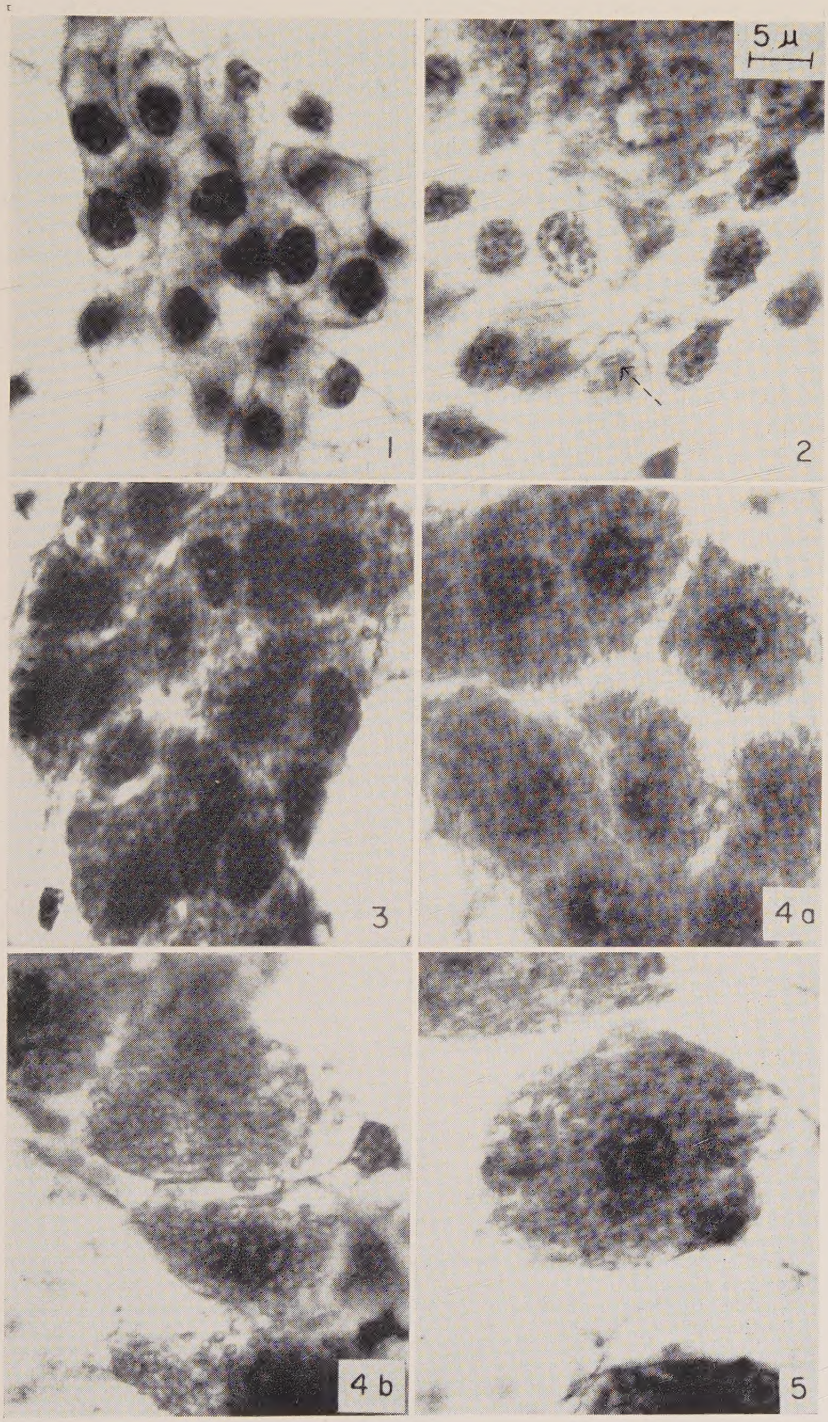
Figures 1 and 5 are photomicrographs of cross-sections of nymphs resulting from feeding the parent generations the two diets. It seemed necessary to only experimentally compare the two diets factor for factor to determine what was responsible for the discrepancy. As might be expected, this task turned out to be less than simple.

Tables 1 and 2 give the constituents of the two diets. They are basically quite similar. The obvious differences are the lack of vitamin B_{12} (cyanocobalamin) in Noland's; the lack of paraminobenzoic acid and vitamin K (menadione) in Gordon's. So the two diets were replicated and to each replicate was added one of the missing vitamins. Lots of 20 newly-hatched nymphs (*Blattella germanica* L.) representing several egg capsules were reared on these diets. When the F_1 generation hatched, samples consisting of 6 nymphs from each of the first 3 egg capsules were prepared histologically. It turned out that none of the added vitamins had any effect on the transmitting ability of the original diets.

Since the two salt mixtures differed in several respects, especially in some of the trace elements, their efficacy was compared by substituting Gordon's salt in Noland's diet and vice versa. The results were positively conclusive: either diet when made with Wesson's salt resulted in an F_1 generation practically devoid of bacteroids while an abundance of symbiotes was transmitted with the other mineral mixture.

When the minerals were expressed on a molar basis, as in Table 3, their differences were readily compared, quantitatively as well as qualitatively. The salts were again replicated in such a way that each one received a concentration of one deficiency, such as zinc or chloride, in the same concentration at which it was used in the alternate diet. In these tests, the salt mixtures were always used only in combination with the other ingredients of Gordon's diet.

Figures 1 through 5 are photomicrographs of mycetocytes in the fat body of day-old cockroach nymphs. The material was prepared by the paraffin ribbon method, cut at 6 μ, and stained with Delafield's hematoxylin and erythrosin-saturated clove oil. Photographs were done with an oil immersion lens of N.A. 1.32, Kodak film M 135, and a Wratten green B filter. The magnification is the same for all as given in the upper right hand corner. Figure 1 indicates the closeness of the mycetocytes when bacteroids are lacking as a result of inadequate manganese and zinc in the mother's diet, as when Wesson's salts are used. Such an aposymbiotic nymph is almost certain to die. Figure 5 shows the size and spacing of mycetocytes that are filled with bacteroids as a result of feeding the mother balanced mineral ratios, as in Gordon's salt mixture, or a natural diet. The numbers of the figures correspond to the categories of bacteroid-transmission, which are explained more fully in the text. Note the two or three bacteroids visible in Figure 2 near the arrow. Figures 4a and 4b are the rosettes or chains referred to as "delayed embryonic development."


5 μ
1
2
3
4a
4b
5

Tables 4 and 5 list the ions or radicals that were tested, together with the results observed in the F_1 generation.

In order to facilitate making comparisons of the sections of the nymphs, an arbitrary classification was set up as follows:

1—apparently no bacteroids
2—very few bacteroids, nearly empty mycetocytes clustered together
3—subnormal complement of bacteroids, mycetocytes clustered together
4—normal complement of bacteroids, mycetocytes close together, forming rosettes or chains
5—normal complement of bacteroids, mycetocytes uniformly dispersed throughout fat body

Table 1. Composition of Gordon's Diet, Stated as Grams per Kilogram Diet

glucose	360.0
vitamin-free casein	300.0
salt mixture	28.0
cholesterol	10.0
linoleic acid	10.0
vitamin mixture	7.0
celluflour	285.0
	1000.0

Salt Mixture:

calcium carbonate	2.000
copper sulfate + 5 H_2O	0.100
ferric ammonium sulfate + 12 H_2O	0.960
manganous sulfate (anhydrous)	0.223
magnesium sulfate (anhydrous)	4.800
potassium sulfate	4.400
potassium dihydrogen phosphate	13.600
sodium bicarbonate	1.700
zinc sulfate + 7 H_2O	0.290
	28.073

Vitamin Mixture:

choline chloride	2.800
thiamine HCl	0.068
riboflavin	0.076
niacin	0.098
calcium pantothenate	0.096
pyridoxine HCl	0.082
inositol	3.600
para-amino benzoic acid	-------
biotin	0.012
pteroylglutamic acid	0.088
menadione	-------
cyanocobalamin	0.002
	6.922

Table 2. Composition of Noland's Diet, Stated as Grams per Kilogram Diet

glucose	320.0
vitamin-free casein	300.0
salt mixture (Wesson's)	40.0
cholesterol	10.0
corn oil	30.0
vitamin mixture	6.0
celluflour	294.0
	1000.0

Salt Mixture:

calcium carbonate	8.400
copper sulfate + 5 H_2O	0.016
ferric phosphate + 4 H_2O	0.588
manganous sulfate (anhydrous)	0.008
magnesium sulfate (anhydrous)	3.600
potassium aluminum sulfate + 24 H_2O	0.004
potassium chloride	4.800
potassium dihydrogen phosphate	12.400
potassium iodide	0.002
sodium chloride	4.200
sodium fluoride	0.023
tricalcium phosphate	5.960
	40.001

Vitamin Mixture:

choline chloride	4.000
thiamine HCl	0.012
riboflavin	0.018
niacin	0.100
calcium pantothenate	0.040
pyridoxine HCl	0.016
inositol	2.000
para-amino benzoic acid	0.050
biotin	0.0006
pteroylglutamic acid	0.005
menadione	0.001
cyanocobalamin	-------
	6.243

The rosettes and clusters of mycetocytes resemble stages in the early embryo. Similar conditions are seen in nymphs in which the bacteroids have been partially eradicated following sulfa drug feeding of the mother. It appears that anything which causes an insufficient number of bacteroids to enter or survive in the egg also causes the embryonic condition to persist beyond hatching. I believe this is because the mitotic division of the mycetocytes is, in part, stimulated by the pressure of the bacteroids; in part, by the endocrine system of the insect (Brooks and Richards, 1955b). If the mycetocytes do not contain enough bacteroids, the divisions which should have occurred in the embryo are delayed until after hatching.

The results were unequivocal with Gordon's salt. The lack of manganese resulted in slow growth and poor survival of the parent generation, followed by aposymbiotic nymphs. The lack of zinc, on the contrary, had no retarding effect on the growth of the parents, while the nymphs were practically aposymbiotic. Nothing else tested had any observable effect on bacteroid-transmission. But the curious thing was the apparent contradiction in the Wesson series. Here *added* manganese permitted only partial transmission of bacteroids; and the addition of zinc had only slight beneficial effects. The histological pictures were as shown in Figures 2, 3, and 4.

The effect of mineral concentration on embryonic development of bacteroids may be expressed quantitatively as follows:

Gordon's salts and modifications			*Bacteroid transmission*
a) 1.478 mM	Mn + 1.000 mM	Zn ⟶	5
b) 1.478 mM	Mn + 0	Zn ⟶	4
c) 0	Mn + 1.000 mM	Zn ⟶	1
Wesson's salts and modifications			
d) 0.053 mM	Mn + 0	Zn ⟶	1
e) 0.053 mM	Mn + 1.000 mM	Zn ⟶	2 or 3
f) 1.478 mM	Mn + 0	Zn ⟶	3 or 4

(*a* and *d* represent the concentrations in the original formulae.)

Table 3. Comparison of Salt Mixtures, Stated as Millimoles per Kilogram Diet

Element or Radical	Gordon's	Wesson's
Al	----------	0.016
Ca	20.000	103.250
Cu	0.626	0.100
Fe	2.000	3.900
H_2	120.220	91.100
K	125.250	155.578
Mg	39.900	29.900
Mn	1.478	0.053
Na	20.220	72.448
Zn	1.000	----------
Cl	----------	136.350
CO_3	40.220	84.000
F	----------	0.548
I	----------	0.012
NH_4	2.000	----------
PO_4	100.000	114.250
SO_4	70.192	30.085

I interpret these results as indicating that manganese is an absolute essential for bacteroid transmission while zinc acts as a synergist to manganese (compare *a* with *c*, and *a* with *b*). There still remains the question of why *b* does not equal *f*.

Referring back to Table 3, we see that there is a great discrepancy in the quantities of calcium in the two mixtures. Expressed as molar ratios, Gordon's has Ca:Mn = 14 and Ca:Zn = 20; while Wesson's has Ca:Mn = 1945 and Ca:Zn = ∞. If calcium acts as an antagonist to Mn and/or Zn, this could account for the discrepancy betwen *b* and *f* above. This possibility was tested by making the appropriate adjustments in the calcium concentration of the two salt mixtures, and it proved to be tenable, as expressed below.

Calcium modifications and Bacteroid transmission

a) Gordon's with 10 × Ca CO_3, so that	Ca:Mn = 140	⟶	4
b) Wesson's with reduced $Ca_3(PO_4)_2$,	Ca:Mn = 1770	⟶	3
	Ca:Mn = 569	⟶	4
d) Wesson's with reduced Ca_3 but increased Mn and Zn	Ca:Mn = 14	⟶	5

So, what at first seemed to be an unintelligible series of contradictions was finally resolved as being the results of stimulation, synergism, and antagonism involving three minerals. The results are summarized in Table 6.

DISCUSSION

Since symbiotic microorganisms are so widespread and vital to the welfare of insects, usually fulfilling a nutritional function, it becomes of peculiar interest to find a reciprocal effect of nutrition on the symbiotes. This underscores the importance of determining the natural relationship between symbiotic partners before attempting to analyze them separately.

Entomologists are gathering increasingly more data on the importance of

Table 4. Changes in Ion Concentrations of Gordon's Salt Mixture

The following increased to:			Bacteroid Transmission to F_1
Al	0.016 mM	(as $KAl(SO_4)_2$)	5
CO_3	104.000 mM	(as $CaCO_3$) (Ca = 84)	5
Ca	60.000 mM	(as $Ca_3(PO_4)_2$) (PO_4 = 140)	5
Na	41.000 mM	(as NaCl) (Cl = 20.7)	5
Cl	136.000 mM	(as NaCl + KCl) (Na = 72 & K = 209)	5
F	0.548 mM	(as NaF)	5
I	0.012 mM	(as KI)	5
Ca	135.000 mM	(as $CaCl_2$) (Cl = 115)	5
Mg	60.000 mM	(as $MgSO_4$) (SO_4 = 90)	5
The following decreased to:			
Zn	zero mM		4
Mn	zero mM		1

Table 5. Changes in Ion Concentrations of Wesson's Salt Mixture

The following increased to:			Bacteroid Transmission to F_1
Cu	0.562 mM	(as $CuSO_4$)	1
Mn	1.478 mM	(as $MnSO_4$)	3
SO_4	70.100 mM	(as K_2SO_4) (K = 196)	1
Zn	1.000 mM	(as $ZnSO_4$)	2
Zn	1.000 mM	+ vitamin B_{12}, 2 p.p.m.	2
Zn	1.000 mM	+ yeast, 1:3 in the diet	2
The following decreased to:			
Cl	zero mM		1

continuing nutrition analyses beyond the growing stages of the immature forms. Egg maturation is not necessarily an unmodified continuation of growth; but from the nutritionist's point of view the two may be entirely separate processes. Therefore no artificial diet must be considered complete unless it can maintain at least one life cycle. In the present study, it was clear that at least two elements, zinc and calcium, in the particular salt combinations studied, had no effect on the growth rate of the insects. But these two elements critically affected egg viability. That the viability was mediated through the metabolism of a second organism is probably immaterial from the standpoint of dietetics.

We may inquire why the intra-ovum stage of the bacteroids is so much more susceptible to interruption than is the mycetocyte stage. Several possibilities come to mind. First, the enlarging eggs are growing at a rate disproportionate to the rate of growth of the insect and therefore may be more metabolically active than the fat body. This would permit a selective concentration of antibiotics in the eggs; or conversely, an early depletion of essential minerals if the supply were insufficient. In either case, the result would be a quite different substrate for bacteroid-growth compared to that in the fat body.

A second possibility is that the bacteroids themselves may be more metabolically active in the egg because of the necessity of their increased multiplication rate to keep pace with the enlarging eggs. In the juvenile insect, the bacteriods have a generation time of about 10 days inasmuch as the mycetocytes divide only once in 10 days. The eggs, on the other hand, increase their volume tremendously in about 7 days prior to each oviposition act; presumably the generation time of the bacteroids is much less than 10 days. Rapid exhaustion from the egg cytoplasm at this critical time of an enzyme activator needed for their own cytoplasmic structure spells the death of the bacteroids.

In either case, whether we are dealing with exhaustion by the activity of the cytoplasmic substrate or by the activity of the organisms themselves, we are reminded of two interesting papers on *Plasmodium*. Trager (1955) found that extracellular forms of the parasite need a more complex molecule than is needed by the parasite in combination with the red blood cell sub-

Table 6. Effects of Zn concentrations and of Ca:Mn ratios on bacteroid transmission (b.t.)

In Gordon's salt, if:	In Wesson's salt, if:
1. Zn = 1.0 mM Ca:Mn = 14 b.t. = 5	1. Zn = 0 Ca:Mn = 1945 b.t. = 1
2. Zn = 0 Ca:Mn = 14 b.t. = 4	2. Zn = 1.0 mM Ca:Mn = 1945 b.t. = 2
3. Zn = 1.0 mM Ca = 20 mM Mn = 0 b.t. = 1	3. Zn = 0 and Calcium is decreased so that Ca:Mn = 1770 b.t. = 3
4. Zn = 1.0 mM and Calcium is increased so that Ca:Mn = 40, 60, or 90 b.t. = 5	4. Zn = 0 and Calcium is decreased so that Ca:Mn = 570 or 380 b.t. = 4
5. Zn = 1.0 mM and Calcium is increased so that Ca:Mn = 140 b.t. = 4	5. Zn = 0 and Manganese is increased so that Ca:Mn = 70 b.t. = 3

strate. Terzian and Stahler (1960) reported that several inorganic materials affect the immunity of mosquitoes to infection by oöcysts. Seemingly this phenomenon is comparable to our results on the influence of minerals on infection of the egg.

The identity of obligate intracellular organisms or bodies is always of great interest and many times indefinite at best. Finding that the bacteroids are stimulated by metallic ions that are known to be activators of enzymes lends support to the belief that they are living microorganisms. The similarity of action of aureomycin and manganese-deficiency gives added support to the theory that tetracyclines chelate manganese and other ions essential to enzymatic protein synthesis in growing bacteria (Burkholder, 1959).

The importance of balanced ratios of minerals and their ability to either enhance or antagonize one another is, of course, nothing new. But the importance of molar ratios is emphasized here to remind the invertebrate physiologist of three facts:

1. The critical mineral constituents of diets must be determined experimentally. The use of mineral salt mixtures designed for feeding vertebrates is illogical.

2. A balanced salt solution compatible with invertebrate tissues (insects in particular) is a pressing need for further progress in tissue culture. The intracellular milieu is far different from that of the hemolymph in several respects (Martignoni, 1960). Analyzing and simulating the blood, even though a necessity, may not be of much help in devising a culture medium for the intracellular microorganisms.

3. Since numerous parasitic organisms are transmitted congenitally, we do not break continuity in shifting our attention from symbiotes to parasites. There may be greater metabolic similarity between an intracellular symbiote and an intracellular parasite than there is between two intestinal parasites.

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The Physiology of Intracellular Parasites*

WILLIAM TRAGER

Three years ago I had the pleasure of being a member of a committee charged with the planning of an exhibit on "Intracellular Parasitism and Symbiosis" for the International Science Exposition of the Brussels World's Fair of 1958. The other members of the committee, all well known to you, were Dr. Clay Huff, who was chairman, and Drs. Francis Gordon, Leslie Stauber, and Emilio Weiss. In our efforts to boil down the many fascinating facts and problems of our subject to the small space and still smaller budget alloted to it we had many prolonged and stimulating discussions. The ideas developed in these discussions inevitably became blended with my own and I wish to take this opportunity to acknowledge my debt to Drs. Huff, Gordon, Stauber and Weiss. As you see, we remained friends through it all.

It has long been known that the cells of plants and animals are often invaded by parasitic microorganisms. Some of these intracellular parasites have attracted special attention because they are agents of disease in man or in animals or plants of economic value. One need only mention the bacillus of leprosy, the rickettsiae of typhus fever, the protozoa of human malaria, the pox viruses, the rust fungi, the slime mold causing club root of cabbage. It is worth emphasizing at the start that some intracellular microorganisms are essential to the well-being of their host. The root nodule bacteria of leguminous plants are an outstanding example; they supply more nitrogen to cultivated plants than does any other single source (Virtanen 1947).

The few examples I have just given already indicate the many diverse taxonomic groups within which intracellular parasitism has arisen. One could go on to name many others and to give details of their complex life cycles and host specificities. But the most interesting aspect of intracellular parasitism is the physiological one. The intracellular parasites represent an extreme of ecological specialization—one organism living not merely in the fluids or body cavities of another but within the still living cells of the host. What special nutrients must the parasite obtain preformed from its host? How do parasites penetrate the cell membrane of the host without disrupting it? How do the parasites influence the metabolism of the host cell? How is it, for example, that penetration of a cell by the protozoan parasite *Leucocytozoon,* or by the slime mold *Plasmodiophora,* is followed by gross enlargement of the host cell and its nucleus? How does it happen that the cells of the root nodules of leguminous plants produce hemoglobin only when they are infected by symbiotic bacteria (Allen & Allen 1954)? Could nucleic acid from the parasite control synthesis in the host? The answers to these and other equally significant questions are not yet aavilable. But it is with questions such as these in mind that I would like to discuss some of the facts already known about the physiology of intracellular microorganisms.

All parasites have had to solve much the same problems in order to exist. Intracellular parasites, like extracellular ones, must have ways of dissemination so that they can reach fresh hosts, and they must have ways of gaining entry into the host. In addition, intracellular parasites must be able to penetrate particular host cells and obtain from these the nutrients necessary for growth and reproduction. Finally, they must have means for leaving the host cell.

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The material for this paper is drawn from a more extensive discussion of the same subject to appear in a chapter by the author entitled "Intracellular Parasitism and Symbiosis" in *The Cell,* J. A. Brachet & A. E. Mirsky, Eds., Academic Press, New York, 1960.

A parasite may enter a cell passively or actively. Viruses of plants are often injected directly into the phloem cells by the stylets of the aphid or leafhopper which serves as a vector. Many parasites are taken up by activities of the host cell, phagocytosis or pinocytosis. Tubercle and leprosy bacilli, and the protozoon *Leishmania donovani* are presumed to be phagocytosed by their host cells. No highly specific mechanisms are here involved. The parasites are taken up much as would be other particles of similar size. The specificity begins with the fact that, whereas dead organic material or nonparasitic organisms would be digested by the phagocytic cell, the intracellular parasites resist digestion and indeed multiply within the cell.

Specificity appears at the outset, however, when the host cell is entered in a manner demanding some activity on the part of the parasite.

Spores of rust fungi will germinate under appropriate conditions of temperature and moisture. If the spore is on a suitable species of plant, the germ tube will then penetrate the cuticle and form a very small intercellular mycelium. Then a special cell is formed, the haustorium mother cell. This becomes closely appressed to a host cell wall which it then penetrates through a fine pore. In this way the fungus enters a host cell, forming the haustorium, which evidently derives nourishment from its host (Walker 1950). The successful completion of this process of penetration, as well as the further development of the parasite, depend on the precise genetic constitution of both the parasite and the host. Extensive genetic studies have been made with strains of rusts and their host plants. There is much evidence to support an interesting hypothesis that for infection to occur there must be both a particular gene for virulence in the parasite and a corresponding gene for susceptibility in the host (Flor 1956). These valuable studies, however, tell us nothing of the biochemical mechanisms underlying this host-parasite specificity.

Since the erythrocytic stages of malaria develop in cells with no phagocytic activity, it must be assumed that the infective forms, the merozoites, penetrate the surface membrane of the red cell by mechanical and chemical activities of their own. They are remarkably host specific. Even in the same animal, different species of malaria show preferences for different types of red cells. *P. vivax* in man preferentially invades reticulocytes (Ferrebee *et al.* 1946). So does *P. berghei* in the rat (Singer 1954). There is good evidence that people heterozygous for sickle cell trait are relatively more resistant to falciparum malaria, thereby accounting for the high prevalence of sickle trait in malarious countries (Allison 1957). Perhaps *P. falciparum* selectively avoids cells with hemoglobin S.

A start toward solving some of these problems was made by R. B. McGhee about 10 years ago. He found that chick embryos infected with the bird malaria parasite *P. lophurae*, could be infected intravenously with washed suspensions of red cells of different species of animals. Four hours later the degree of penetration of these introduced red cells, which differed morphologically from those of the embryo, could be determined quantitatively. The most interesting result came from a study of rat erythrocytes from animals of different ages (McGhee 1953.). When introduced into infected chick embryos, the red cells of weaned or older rats were completely immune to *P. lophurae* whereas the red cells of newborn rats were quite susceptible (Table 1). If we knew the biochemical basis for this difference, we would have made significant progress toward understanding host-parasite specificity at the cellular level. The isolation of pure suspensions of malarial mero-

zoites and study of their enzymatic activities might provide further basic information concerning the penetration of host cells.

Once a parasite is inside its host cell it must find there the conditions suitable for its growth and reproduction. The parasite must obtain, either directly from its host cell, or from the external medium via the host cell, all its essential nutrients. Since most intracellular parasites have not yet been grown apart from living host cells, the study of their nutrition and metabolism is fraught with difficulty. Three approaches have been used: (1) study of the metabolism of the host cell-parasite complex; (2) short term metabolic and chemical studies of surviving parasites freshly removed from their host cells; (3) longer term studies of parasites removed from their host cells, involving attempts to substitute a non-living culture medium for the host cell. Results obtained by the first method, study of the parasite-host cell complex, are not easily interpreted in terms of activities of the parasite itself. We know that the metabolic activities and chemical composition of an infected cell are not merely the sum of those of the parasite and those of an uninfected cell. Hence from studies of the complex we can draw conclusions only about the complex, not about the parasite itself. Some of these conclusions are, nevertheless, of considerable interest and importance.

Becker and Smith (Becker & Smith 1942) first showed that the intracellular coccidian parasite *Eimeria* developed better in rats on an adequate diet than in rats fed a diet deficient in pantothenate. Soon after, I found that the malaria parasite *P. lophurae* survived longer in erythrocyte suspensions kept *in vitro* in the presence of added pantothenate (Trager 1943). There can be no doubt that pantothenic acid must be supplied to the complex malaria parasite plus host red cell in order for the paarsite to develop. Other such essential factors are para-amino benzoic acid, ascorbic acid, thiamin, riboflavin, biotin and methionine.

It is noteworthy that altogether parallel results have been obtained with virus-host cell systems. For example, when T 2 bacteriophage developed in *E. coli* in a synthetic medium of salts, lactate and ammonium, the latent period was much longer and the burst size much smaller than in a broth medium (Cohen & Fowler 1948). The addition to the simple medium of 12 amino acids and adenine gave phage development nearly as good as that in broth. To support good growth of psittacosis virus in tissue culture the medium had to contain amino acids and water soluble vitamins (Heggie &

Table 1. The Comparative Susceptibility of the Erythrocytes from Rats of Various Ages. When Introduced into Chick Embryos Infected with *Plasmodium lophurae*[a]

	Time, in hours, after foreign cell Introduction		
	0		4
Age, in days, of donor animal	Parasites per 10,000 chick erythrocytes	Number rat cells per 10,000 total cells	Parasites per 10,000 rat erythrocytes
I	2100	2800	98
7	1125	3200	19
13	1973	3400	39
17	1000	2900	8
21	2700	2800	7
30	2500	3700	5
40	1800	2600	0.4

[a]The influence of age of the animal upon the susceptibility of mammalian erythrocytes to infection by the avian malaria parasite *Plasmodium lophurae* (McGhee, 1953).

Morgan 1956). Similar results have been obtained with a number of other viruses. As long as the parasite is developing in its host cell we have no way of knowing whether the required metabolite is utilized directly by the parasite or whether it is used by the host cell to synthesize some other material which in turn is essential to the development of the parasite. A case in point is provided by some results with the bird malaria parasite *P. lophurae*. You will remember that this organism appears to require pantothenate when it is developing within host erythrocytes. When the organisms were removed from their host cells, however, and maintained extracellularly in an erythrocyte extract medium, their survival was favored by the addition of coenzyme A to the medium (Trager 1954). Pantothenate had no effect. The results fit the hypothesis that the parasites satisfy their requirement for an external source of preformed CoA by taking it from the host red cell, which in turn requires an external source of pantothenate to enable it to manufacture the CoA.

The metabolism of intracellular parasites removed from their host cells has been studied with only a few kinds of parasites. In general viruses, even the larger ones, have not been found to have any metabolic activity apart from their host cells, and it has been concluded that they lack oxidative enzyme systems. For the larger viruses especially, this conclusion must be treated with some caution. We have to remember that rickettsiae likewise appeared to have no respiratory activity until Bovarnick and Snyder (1949) found an appropriate substrate-glutamic acid. Cytochrome C reductatse has now been demonstrated in the large meningopneumonitis virus (Allen & Bovarnick 1957).

Most of the work of this type has been done with leprosy bacilli, rickettsiae and malaria parasites. Murine leprosy bacilli free from host tissue have an endogenous respiratory activity which falls off rather rapidly at 37°C; it is 60% gone, as is the infectivity, after 24 hours in even the most favorable media (Hanks 1954). Quite unlike the situation with cultivable mycobacteria, the respiration of leprosy bacilli *in vitro* is not stimulated by any of the substrates so far tried.

Oxygen consumption by suspensions of isolated rickettsiae occurs in a medium of high K content and depends on glutamic acid and certain other substrates. The rickettsiae have enzymes of the citric acid cycle (Bovarnick & Miller 1950) and carry out a small but significant phosphorylation accompanying the oxidation of glutamate in the presence of adenosine diphosphate, hexokinase and glucose (Bovarnick 1956). This phosphorylation was increased by the addition of diphosphopyridinic nucleotide (DPN) and CoA.

These two coenzymes are of considerable importance to rickettsiae. If purified preparations of typhus rickettsiae are held 18 hours in isotonic salt solution at 0°C. they lose their toxicity for mice, their respiratory activity and their infectivity for chick embryos. Fifty to 100% of the lost activity could be restored by three hours incubation at 33°C. in presence of DPN (Bovarnick & Allen 1957). The extent of restoration was favored by the further addition of CoA. Even under the most favorable conditions, however, the viability of rickettsiae maintained axenicly must still be measured in hours rather than days.

Malaria parasites can be removed from their host cells in relatively undamaged state by hemolysis with saponin or with hemolytic antiserum-complement system. Such parasite suspensions when first prepared have an oxygen consumption of about 50-75% that of intact infected cells (Speck *et al.*

1946). The parasites appear to be fully equipped with enzymatic mechanisms for the metabolism of glucose. These enzymes, however, may not all be produced by the parasites themselves. We have to be especially careful in view of the recent demonstration by electron microscopy (Rudzinska & Trager 1957) that the parasites engulf portions of host cell cytoplasm.

It is obvious that axenic cultivation of an obligate intracellular parasite would help solve many fundamental problems in the physiology of these organisms. Although this goal has not yet been attained, we have had partial success with the bird malaria parasite *P. lophurae*. This organism removed from its host duck erythrocytes can be kept alive and developing *in vitro* for periods up to four days at 40°C. (Trager 1957). This extracellular survival and development depend on duck erythrocyte extract and the cofactors pyruvate, malate, ATP, DPN, CoA and folinic acid. Note the requirements for coenzymes of both malaria parasites and rickettsiae. Such a requirement for an external source of coenzymes, which in nature occur chiefly within living cells, could in itself be sufficient to make intracellular parasitism obligatory.

On the basis of the very incomplete information now available we may draw the following picture of the utilization of host cell substance by different types of intracellular parasites. Malaria parasites ingest cytoplasm of the host red cell by a process of phagocytosis. They digest the hemoglobin it contains, thereby satisfying the bulk of their N requirements. They utilize carbohydrate and the intermediary metabolites such as pyruvate and malate. They obtain from the host cell several coenzymes and certain non-dialyzable materials. Perhaps these are enzymes. Rickettsiae secure from the host cell the oxidizable substrate glutamic acid, certain coenzymes, and no doubt numerous other factors as yet unknown. Viruses appear to require much, though not all, of the synthetic machinery of the host cell, redirecting it in such a way as to result in synthesis of additional virus.

The way intracellular parasites reproduce varies with the taxonomic group of microorganism: it may be by budding as in symbiotic intracellular yeasts, by binary fission as in leprosy bacilli, leishmanias and rickettsiae, or by schizogony, so well illustrated in the erythrocytic cycle of malaria. In any case, reproduction within the host cell sooner or later makes it necessary for the parasites to escape from the cell in which they have developed and invade new cells. Some intracellular parasites manage to do this without ever leaving the sheltered environment of a living host cell. Thus the non-pathogenic kappa of *Paramecium* is transmitted to the daughter ciliates at each cell division. Certain intracellular symbionts of insects are brought by their host cells into the oocyte of the female host. Here they are later incorporated into special cells of the developing embryo.

But most intracellular parasites have to leave one cell to invade another. This may be accomplished by destruction of the host cell; bacteriophages and erythrocytic malaria parasites escape at the time of lysis of the host cell. So do numerous other viruses and Sporozoa. Other intracellular parasites are released a few at a time through the surface of the still intact living host cell—perhaps by a kind of reverse pinocytosis. This method is well illustrated by eastern equine encephalitis virus (Bang & Gey 1952) and by rickettsiae (Schaechter *et al.* 1957).

And so we have carried the parasites back full cycle to where they must again invade a cell. What, meanwhile, happens to the host cell? If the parasites escape from their host cell by its lysis the end result of the infection

is of course destruction of the host cell.

Bacterial viruses, many viruses of plants and animals, many kinds of intracellular protozoa, rust fungi, produce changes in the host cell which lead to its death and dissolution—but not until after the parasite has completed its development. How the parasites bring about this result is not known. It is not enough to say that the parasite has consumed the interior of the host cell, for this is not always the case. Moreover, a cell may be filled with parasites and yet continue for a time to function in a seemingly normal way. This is beautifully illustrated in Huff's moving picture of chick embryo cells in tissue culture infected with the exoerythrocytic stages of malaria. Cells filled with parasites continue to move about and may even divide (Huff *et al.* 1960).

With the bacterial viruses and the erythrocytic stages of malaria, since cell destruction does not occur until the infective forms have been produced, it might be that the same enzymes of these forms which enable their penetration into a new cell are also responsible, when present in large amounts, for the dissolution of the old host cell. Thus the old cell would be specifically destroyed by the infective forms of the parasite thereby enabling them to reach new susceptible cells.

Other readily apparent effects of intracellular parasitism are stimulation to increase in size, to an increased rate of multiplication, or to both, sometimes followed by neoplasia. Hypertrophy of the host cell is strikingly illustrated in infections of ducks by the protozoon *Leucocytozoon simondi* (Huff 1942).

Hypertrophy and also hyperplasia occur in clubroot of cabbage. This disease, first studied by Woronin (1878) and one of the first intracellular parasitic infections to be described in detail, is caused by an ameboid slime mold. Meristematic cells of cabbage invaded by these small amebas are stimulated to excessive growth in size and to excessive multiplication. The stimulus to hypertrophy and hyperplasia travels slightly in advance of the invading microorganisms, indicating the formation of diffusible growth-stimulating substances either by the parasite or by the infected host cell (Kunkel 1918).

The beneficial effects of some intracellular microorganisms and the intricate relationships between such symbiotes and their hosts have already been discussed earlier this morning by Dr. Brooks in connection with her own recent and striking work.

We may expect that a better understanding of intracellular parasitism will develop together with better understanding of cellular physiology. Each field will draw from and contribute to the other. Discovery of the biochemical bases for host-parasite specificity at the cellular level would give important information concerning the subtle structures and materials which distinguish different kinds of related cells. Particularly valuable would be knowledge of the special nutritional factors which intracellular parasites derive from their host cells. Just as free-living organisms of various kinds served as indicators for the discovery of vitamins, each of which opened a chapter in cellular biochemistry, so it seems likely that intracellular parasites will serve as indicators of additional substances of great physiological significance—substances which most cells can make for themselves but which intracellular parasites must obtain preformed from their host cells.

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A Goal for Parasitologists

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A celebration of this sort means many things. It should be a time of recollection and assessment of the years past, not only of the society itself, but of the field it represents as well. I had thought to obtain notes from Leuckart's lectures to his students and from H. B. Ward's to his for a comparative study of today's parasitology. I quickly realized that unless I had this year's notes from many members of the Society, not much of a comparison could be made. I also recognized that protozoological parasitologists would be stressing their interests, nematological ones theirs and entomological ones theirs, further complicating the task I had hoped to carry out. Obviously, a teacher teaches best from his own interest and enthusiasm, but just as obviously, this was no aid to me. I then considered the textbooks in use as clues and once aagin was brought to the realization how limited the aids to teaching parasitology are. Most of the textbooks available are clinically oriented as they should be for teaching in medical or veterinary schools. I have even used them myself in my undergraduate courses to test their usefulness. Of those remaining that might be called texts, probably few use Baer (1952), Caullery (1950), Hegner et al (1938), Rothschild and Clay (1952), Lapage (1951), or Pearse (1942), and probably few have attempted Cameron (1956) as yet. This leaves only Chandler (1955), which of course, I also continue to use between the times of experimentation with other sources. Yet, from the first edition of Chandler to the most recent one, half the life span of this Society, little change of emphasis is indicated, even to

the continued inclusion of the spirochetes. The point I am trying to make here is not that the spirochetes are not parasites, but that only Cameron logically includes viruses, bacteria, fungi, etc. in his discussion. Obviously, this is no criticism of Chandler or his book. Nothing so successful and useful, to me also, as his book could be basically wrong. It has admirably served the needs of our field for many years. Nevertheless, in the relative lack of attention to whatever principles there are in the field of parasitism, with so much emphasis on human aspects of infectious disease, it has its limitations for departure from the traditional serially, even phylogenetically, descriptive treatment of the field.

After rereading Dr Aurel Foster's (1960) presidential address to the American Society of Parasitologists last year, I also decided not to stress the other aspects of accomplishment of the members of this Society and the field of parasitology which he so ably summarized. That parasitologists of so many sorts have made so many contributions to human welfare in the broadest sense of that phrase is without question. I even agree, as he points out, that had we as a group not been so useful to society, such a history as we now can claim might not have been so glorious. Perhaps of his treatment I should repeat for you only his statement that "parasitology has become of age" because this statement not only summarizes the past adequately, but sets the stage for a discussion of what might lie ahead.

Dr. Foster's presidential address, Dr. Clay Huff's (1956) earlier one also, the study and report of the Huff Committee (Huff et al, 1958) on teaching a course in parasitism and the statement of Dr. Clark Read (1958) in the A.I.B.S. Bulletin advocating a "Science of Symbiosis" indicate that many minds in our discipline are concerned about our present status and are wrestling with the problems of the future. In addition, there are certain disquieting notes. For example, the de-emphasis of parasitology in our medical schools since World War II, the turn of the phrase from Tropical Medicine as we thought of it to Medicine in the Tropics, and the knowledge that several undergraduate departments of biology are not replacing parasitologists with others of our kind. The awesome hope that human malaria may shortly be eliminated from the earth suggests that further great accomplishments of similar nature may be but a bit more distant in the future. This might even increase our disquietude if all we had to offer was assistance in disease control in man or beast. I am happily confident in Dr. Foster's belief in the potential greater usefulness of parasitologists to their discipline and society, but should we not ask then what our goals might be in the years to come to keep that record safe.

I thought that one way to start might be to chide myself for what I have not done, or for what I have not been. An examination of the program of this afternoon's meetings startled me, and I suspect there are a few others in the audience facing the same dilemma, though not so acutely as I. Unfortunately, I know too little about systematics and nomenclature and even less about nematodes of plants to contribute to these sessions. Pretension of significant knowledge of chemotherapy would be impossible in the presence of Drs. Harwood and Otto. Doctors von Brand and Bueding, and also my colleagues at home, know me too well to support any claim of mine as a physiologist. Perhaps Drs. Sadun and Oliver-Gonzalez, out of the kindness of friendship, will admit me to their session on immunity. And yet, I ought to belong somewhere since I still like best to call myself and to be called a parasitologist.

Since parasitologists are so many different kinds of workers, we may be confident that the practical problems arising in our field will eventually be solved. Here you see my emphasis is quite the other extreme to Dr. Foster's. Judging by our history, there have been ready hands and heads to tackle these problems. It may even be that we have subserved others too well, leaving more basic problems still unsolved. For the field and for some of us in it, there is plenty of justification that these continue to be ample goals. Of more concern, however, is the need to increase the tempo of our search for basic principles or for what Dr. Huff has just called the closing of the gap between the organismic and atomic levels of parasitology. To obtain these we must have more information on the subtle details of the relationship of parasite in its host. We must encourage much more conscious effort in this direction. I do not care whether it is accomplished by means of Clark Read's "Institute of Symbiosis," or by his inter-disciplinary committee, by the individual investigator or teams of investigators as implied by Huff's (1959) argument for continuing basic research on malaria because it is such an excellent example of intracellular parasitism or by Foster's stress for investigation of "obscure patterns of fundamental phenomena." Perhaps what is needed is for more of the teachers among us to lead students in such ways that they can take these approaches to the host-parasite relation and come back with synthesizing accomplishments. And I do not mean the jumping on a band wagon rather ill prepared as von Brand warned recently about physiology or merely use of the tricks of apparatus or technique, like Warburg or immunoelectrophoresis, which may delude one into thinking this is the goal itself. But I do mean the application of every effort to bring the best of all kinds of procedures and attitudes to bear on the parasite and the parasitism.

In some respects our colleagues of long ago were closer to the goal than many of us today who have consciously gone off to explore interesting sidelines. Read again van Beneden's "Animal Parasites and Messmates." (1876). Or even better, read Keeble's "Plant-Animals: a study in symbiosis" (1910). I have long wondered that so little has been added over the years to this interesting account of the relationship between a host and its uninvited guests. Indeed, the one who reinvestigates this particular relationship may find a part of the solution to the grand problem Read (1960) posed for a space-age parasitologist of the next decade.

If I wanted to, I could not offer a detailed program, so I will make no attempt at it. Also, it is too important that no specific reference of mine be taken as a point of argument which might detract from the constructive thinking needed to formulate the many plans needed to reach the goal. I was tempted to comment on the papers of Brooks and Trager, just heard, since I admire so much the work of each, but I decided that even this might be subversive to my fondest hopes.

I have taken some pains to ridicule myself in order to demonstrate that I certainly intend no offense by my thoughts or words to any of you in my phrases. That which we are seeking—the best future for parasitology—is too important for petty thoughts or acts.

A recent statement makes good introduction to my next thought. At the close of his monograph on digger wasps, H. E. Evans (1957), a well-known systematist, states that, "What a species *is*, ultimately, is not what its external features happen to be, but how it lives, and most important of all, how it came to be. Now that most species of animals have been described, it is time to find out what they are." And he has written of some of his attempts to find

out what digger wasps are. As Foster has hinted, we have come of age to face this in our own field. Or to put it differently, neither systematics nor immunology nor physiology alone is the goal of parasitology. Each of these can be pursued for their own good ends with even better results in other areas with other living systems than the parasitic ones. As examples in the field of immunology, if I may return to it, compare erythrocyte agglutinations with agglutinations of coccidial merozoites, compare pneumococcal polysaccharide quantitative precipitation with precipitation tests in parasitic infections, and note that we do not even have a counterpart for the classical toxin-anti-toxin reaction. The goal of a parasitologist is the study of parasites and their relations with their hosts; what they are, how they live and how they came to be at organismic, cellular and molecular levels. There will always be a need for this kind of pursuit of knowledge and it will transcend the time, even if it should come, when the harmful consequences of parasitism to mankind are eliminated. For some of us who have digressed too far, rededication to this major objective is necessary.

The other part of the goal is aimed at the teacher. Huff's Committee made but a small start toward this. The need here is for rearrangement and new presentations of material in ways not yet dreamed of, but which will lend a hand in the delineation of new lines of attack and new problems to be solved. Such action will attract new, active young minds and also encourage and excite them to ask the kinds of questions and to be as bold and daring as Read's space parasitologists will need to be to obtain the answers for the future.

In closing I have a suggestion for both teaching and research; namely, the application of so-called "brainstorming" committee activity to these problems. While I know this is done by industry and government research units where practical solutions are sought, and while I think it is implicit in Read's plan for an Institute of Symbiosis, it might be initiated by our societies to consider approaches to basic problems. And for the teaching, an extension of the Huff Committee idea, every couple of years, if the committee included one or more of the younger teachers willing to put the suggestions to trial, might lead as in the A.I.B.S. biology curriculum study to new approaches to teaching which eventually would help us reach our goal. It might even be that support from N.S.F. or N.I.H. could be obtained specifically to buy part of the time of some teacher to allow him the opportunity to exploit the ideas developed by such brainstorming committees. I can visualize different types of courses (ecological, physiological as well as more traditional) which might be put to test.

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Afternoon Meetings

CONCURRENT GROUP DISCUSSIONS

GROUP I. IMMUNITY TO PARASITES: This panel discussion, attended by 44 members and guests, was guided by L. A. Jachowski. No opposition was raised to the definitions of immunity as resistance to infection and of serology as the study of antigen-antibody reactions.

Innate immunity is recognized in parasitology, but its mechanisms are unknown. This resistance can be modified by injecting serum from susceptible hosts, by modifying the diet of the hosts. Moreover, selective culturing of the parasite can adapt it to hosts who are normally resistant.

Acquired immunity does not appear to be related to the recognized antigen-antibody systems. Somatic antigens, regardless of purity, generally produce no resistance. Metabolic antigens produce equivocal results. Infection with irradiated larvae impart significant reduction in challenge infections with homologous parasites. Some evidence of cross protection between unrelated parasites was presented.

The antibody in serologic reactions is usually in the gamma globulin fraction of the serum. When fractionated further, a fast-moving component of this fraction is most active.

Somatic antigens are under intense study. Acid soluble and insoluble proteins, glycoproteins, and polysaccharides are being isolated and chemically standardized.

Metabolic antigens are being explored at an accelerated pace. Little progress has been made in identification of these products.

Emphasis was placed upon: 1. The assay and standardization of antigens, 2. The need for new approaches to serologic problems with considerable interest in "free antigens," and 3. Use of agargel techniques to identify antigen-antibody systems.

The general impression was that considerable progress is being made in isolated studies, but that these efforts are not being focused upon a single general goal. No suggestions were presented on how this coordination might be achieved. J. OLIVER-GONZALEZ AND R. I. ANDERSON, Moderators.

Group II. nematodes of Plants: A lively discussion among 19 participants ranged over the whole field. Questions were raised concerning history of the Society, life cycles, taxonomy, biochemistry, technique, ecology, host-parasite relations, behavior, and control. It was very clear to all that nematodes of plants are receiving an increasing amount of attention from scientists, both as objects of immediate economic concern and as illuminating examples of parasitism.

Present were: Baker, Buhrer, Christie, Cobb, Esser, Fielding, Friedman, Golden, Hannon, Harrison, Hirschmann, Krusberg, McCoy, Schindler, Springer, Taylor, Thorne, Triantophyllou. Victor Dropkin, Chairman.

Group III. Parasite Physiology: The first part of the session was devoted to the discussion of the prevalence of fermentative processes in protozoan and metazoan parasites whose habitats have a high oxygen tension. For example, anaerobic reactions predominate in adult *Schistosoma mansoni;* *Trichinella* larvae produce large amounts of organic acids as fermentation products despite the presence in these organisms of enzymes catalyzing the complete oxidation of carbohydrate *via* the tricarboxylic acid cycle. It was pointed out that parasitic habitats may either favor the induction of fermentative reactions or inhibit the development of some oxidative enzymes. No evidence is available indicating the genotypic loss of aerobic metabolic faculties; on the other hand, the origin of fermentations in certain parasites by mutations regulating the ratio of fermentations/oxidations cannot be excluded. There was agreement that a genetic approach is required in an attempt to investigate these problems of biochemical evolution and adaptation.

During the second part of the session some recent findings in the field of parasite physiology were reported. Dr. Vernberg has correlated the optimal temperature for the oxygen uptake of adult intestinal trematodes with their host. This optimum was lowest (30°C) in a fish trematode, highest (40°C) in a bird trematode, and intermediate (34°C) in a turtle trematode. Dr. Bowman has found that trypanosomes (*T. rhodesiense, T. gambiense, T. cruzi*) are unable to utilize trehalose and that addition of this disaccharide to the medium does not restore the infectivity of old trypanosome cultures. In the course of this investigation a specific trehalase of blood plasma was uncovered. Dr. Entner has demonstrated in the reproductive system of *Ascaris* the presence of systems concerned with the synthesis of both DNA and RNA. Dr. Symons has investigated physiological changes in the intestine of rats infected with *Nippostronglylus muris.* This infection produces a hyperplasia of the intestinal mucosa and is associated with a marked reduction in the absorption of perfused glucose, of histidine, and of sodium in the upper intestine, while absorption is increased in the lower intestine. Protein digestion is decreased, probably because of the dilution of proteolytic enzymes caused by the large amount of fluid accumulating during the infection. Dr. Saz discussed the mechanisms involved in the formation of the fermentation products of adult Ascaris: succinate, propionate, acetate, tiglate and alpha-methylbutyrate. Evidence obtained by the use of C^{14} labeled precursors and from the properties of some isolated enzyme systems of *Ascaris* muscle indicates the occurrence of the following reaction sequences: pyruvate is formed from carbohydrate *via* the Embden-Meyerhof scheme of phosphorylating glycolysis. Succinate is produced by CO_2 fixation into pyruvate, probably by the action of the malic enzyme, giving rise to malate, dehydration of the latter to fumarate, catalyzed by fumarase, and subsequent reduc-

tion by DPNH of fumarate to succinate. The latter reaction is catalyzed by a particulate enzyme of *Ascaris* muscle. Succinate is decarboxylated to propionate while acetate originates from the oxidative decarboxylation of pyruvate. The branched chain carbon skeleton of alpha-methylbutyrate is produced by a condensation of the carboxyl carbon of acetate with the alpha carbon of propionate, giving rise to alpha-methylacetoacetate which, on reduction and dehydration, yields alpha-methylcrotonate (tiglate) whose reduction product is alpha-methylbutyrate. In the course of these reductions DPNH, produced as a result of the formation of pyruvate and of acetate, is reoxidized to DPN. Dr. Fairbairn stressed the significance of CO_2 fixation for the metabolism of various parasites. ERNEST BUEDING, Co-Chairman.

GROUP IV. CULTURE OF PARASITES: The session on the "Culture of Parasites" was attended by approximately 50 people. An opening statement was made by Dr. P. Weinstein, and the meeting was then turned over to Dr. G. H. Ball, who served as moderator.

The growth of the "parasitic" stages *in vitro* of several species of nematodes from domestic animals was discussed, with the indication that rapid progress was being made in this area. Complete development, however, has not yet been achieved, inasmuch as only fourth stage or immature fifth stage worms have appeared in cultures. Continued progress was also reported in determining the nutritional factors which bear upon the *in vitro* differentiation of the sexually mature stages of various nematode parasites of rodents.

The report of a chemically-defined medium which will support the growth of a nematode parasite of an insect signifies an important advance in this area.

Information on the use of *in vitro* culture methods to assess the effects of serum and tissue extracts from immunized animals on parasitic nematodes was presented.

The lack of success in obtaining growth of explants of parasitic helminths in media capable of supporting the growth of the intact organism was discussed. It was suggested that knowledge of the internal milieu of the worm was basic to the solution of this problem. In the case of nematodes, it was also pointed out that the constancy of cell numbers characteristic of these organisms might constitute an inherent biological obstacle to the culture of tissue explants.

The use of organ culture for the *in vitro* study of parasitic amoebae was presented, and the many advantages of a modified Breckenridge chamber for the development of the intracellular stages of malaria parasites were described.

The various chemical and physical factors which might be involved in the interesting enhancing effect of very low concentrations of agar on the growth *in vitro* of parasitic protozoa was discussed, and it was suggested that further work be done to elucidate the role of this substance on protozoal growth.

The effects of various salts and sugars on helminth survival *in vitro* was presented. The ratios of various ions, e.g. Na:K are of considerable importance, as well as the ratio of magnesium to dextrose. It was suggested that the inorganic salt requirement of many helminths *in vitro* might not be satisfied by the usual mammalian-type balanced salt solutions, and that a detailed study of the requirements for inorganic ions was needed. PAUL P. WEINSTEIN, Chairman.

Group V. Systematics and nomenclature: Out of a total attendance of approximately 50 individuals, 47 signed the roster or were known to be present.

The systematics and nomenclature group consisted not only of local helminthologists and guests but two members of the Society, from as far away as Michigan and Florida, were accompanied by their graduate students. Other states represented were California, New York, Pennsylvania, West Virginia, Wisconsin, Kentucky, Georgia, and South Carolina. Other countries represented were Canada, India, Japan, Norway, Pakistan, and Poland.

The group was most fortunate in having an experienced panel in both systematics and nomenclature, consisting of LaRue, Sabrosky, Stunkard, Wharton, and Yamaguti.

The moderator, after welcoming the members of the group and introducing each of the panelists, announced that it had been decided in advance of the meeting that there would be no formal program consisting of prepared papers to be read. This would be in keeping with the style followed by the Society in the first 25 years of its history.

Each individual was to feel free to suggest a topic for discussion or to present questions to the panel. The first topic to be discussed was one of generic synonymy, concerning the status of *Distomum* Diesing, 1850 and *Distoma* Retzius (1782) 1790.

The problem was an easy one as it was brought out that Diesing in 1850 had used *Distomum* for *Distoma* and that *Distoma* is *Fasciola* Linneaus, 1758, renamed. Hence, the type for all three generic names is *hepatica* Linneaus, 1758; thus, by priority, *Distomum* and *Distoma* are synonyms of *Fasciola*.

Under zoogeography were questions pertaining to the differentiation of closely related species from different continents or whether one should regard such differences, that might be noted, as only variations of a single species. In this connection, the size and shape of spines or scales for the evaluation of species of trematodes was discussed. It was pointed out that in following the development of spines from metacercaria to adult in *Paragonimus* the groups of slender spines as seen in mounted specimens come from the splitting of the large spines.

How to describe a new species was discussed by several members of the group. Some preferred to base the diagnosis on the holotype, employing the telegraphic style. Some preferred a mixture, employing full sentences and a telegraphic diagnosis. Others were opposed to establishing a species based on a single specimen.

In describing a new species it was pointed out that the author should designate, from among the several specimens that he has himself examined, one as a holotype and the remainder as paratypes. The type host and type locality should be given and if some of the paratypes are from different hosts and localities the information should be recorded. The types should be deposited in some museum, so as to be available to other workers, and the catalogue numbers assigned to the specimens noted in the publication.

The term genotype came up next for discussion. It was pointed out that in the new Code there would be a recommendation that this term be abandoned in systematics as it had been appropriated by geneticists for an entirely different meaning. Systematists in place of genotype should now use type species.

At the mention of the New Code, a member of the group wanted to know if subspecies were accepted and was informed by a panelist that the Code did not interfere in Zoology, but was concerned with the names given to the

various taxa, including subspecies. It was pointed out, however, that names given to varieties, etc., below the rank of subspecies, after the New Code is published probably in 1960, would have no standing in nomenclature.

The endings of supergeneric names and what appears to be lack of uniformity, to some workers, in forming subfamily and family names came up for discussion. We were informed that the New Code will recommend *-ini* for the ending of a name of a tribe and *-oidea* for the ending of a superfamily.

The proper spelling of subfamily and family names based on generic names like *Echinostoma* and *Paramphistomum*—Echinostomatinae, Echinostomatidae, and Paramphistominae, Paramphistomidae—have been very confusing to many of us and we have often wondered why we should spell one with an *at* and not the other. This apparent lack of uniformity was cleared up by a member of our panel. It was pointed out that the stem of "stoma," a word of Greek origin, is *stomat,* the subfamily and family endings being *-stomatinae* and *-stomatidae;* the stem of "stomum," not Greek but Latinized, is *stom,* the subfamily and family endings being *-stominae* and *-stomidae.*

The next topic presented for discussion was how should one name larval stages, especially trematodes. It was pointed out that for more than 50 years we have had in the old Code, under Article 8, a recommendation that certain biological groups which have been proposed as collective groups, not as systematic units, may be treated for convenience as if they were genera, but they require no type species. Examples: *Agamodistomum, Amphistomulum, Agamofilaria, Agamomermis, Sparganum. Distoma* and *Cercaria* are also used as collective group names. The International Commission of Zoological Nomenclature, at its recent meeting (1958) in London, decided to have inserted in the New Code a clear statement that would permit specific names having their origin in combination with names given for collective groups to compete for priority with specific names having their origin in combination with systematic genera.

In connection with the discussion on *Cercaria* it was brought out that authors instead of giving names to new *Cercaria* numbered them. Should such procedure be followed and, if not, how should the species, known only by a number, be handled? Discussion brought up the fact that Cercariae should be named, not numbered. We were then reminded that cercarial specimens, when preserved as types, are almost worthless for future comparison; that, in most cases, no attempt is made to preserve specimens. Instead of describing only the cercarial stage of a trematode an effort should be made to solve the life history of the species. Then, if the species is found to be new, an adult specimen would be available for the type.

The moderator next introduced two members of the personnel staff of the Index-Catalogue of Medical and Veterinary Zoology. The following "pet peeves" were called to our attention: (1) Some workers, in naming new species, mention several genera and give the same specific name, e.g. *americanum,* under each. Future references to these can be very confusing and difficult, particularly in the case of new combinations. Be more original in forming specific names; (2) in making new combinations state in the paper that it is a new combination, and moreover, spell out new combinations rather than just indicating them; (3) lumping together the host and parasite records can be very confusing, examples: 5 species of ticks on 5 species of rodents were found. This is very difficult for the indexer as are lumped geographical records of parasites; (4) avoid straddling the fence in papers,

example: This may be considered a new species, a new combination, synonym, etc.

A number of complex hypothetical questions were proposed for discussion, but the panel thought it best to study the original papers pertaining to each case involved before offering an answer.

A lively time was had by all participants and more topics could have been discussed, if time had permitted. Present were: Roy C. Anderson, W. M. Becklund, Bernard Bezubik, Eugene Biegalman, Burton J. Bogitsh, J. Robert Buchheit, Thomas C. Cheng, May Belle Chitwodo, A. B. Cowan, Raymond T. Damian, D. DeGiusti, Donald W. Dery, Mildred A. Doss, T. T. Dunagan, S. P. Gupta, John E. Hall, E. V. Hoffman, Bruce E. Hopper, Judith M. Humphrey, Kathleen L. Hussey, Donald Johnston, Newton Kingston, George R. LaRue, D. K. Lawless, Jean Lee, John T. Lucker, Austin MacInnis, Allen McIntosh, Gary E. Marker, Betty June Myers, Benjamin P. Monoco, Pir Nasir, R. E. Ogren, L. E. Peters, Curtis W. Sabrosky, E. L. Schiller, Curt R. Schneider, S. A. Sher, Doys A. Shorb, E. V. Sillman, Joetta Smith, R. B. Short, Horace W. Stunkard, Fred Thompson, Rolf Vik, George W. Wharton, Satyu Yamaguti. Allen McIntosh, Moderator.

Group VI. Chemotherapy: The discussion of chemotherapy appeared to fall under three main headings, as applied to (1) public health and human medicine, (2) veterinary medicine and livestock, and (3) nematode parasites of agriculturally important plants, with the inevitable overtones on the needs and the problems of official evaluations and regulations. Introductory statements were made by the chairmen and the members of the informal panel, Harold W. Brown, Paul D. Harwood, Dewey J. Raski, Aurel O. Foster, Frank Enzie, and Charles G. Durbin. Most of the 35 people attending participated in the discussion.

It was brought out that the standards and requirements for the introduction of a new chemotherapeutic agent are much higher than when this Society was formed fifty years ago. The success in the control of some of the major diseases has turned attention to some less prevalent diseases and to some of the less fatal infections as well as giving renewed attention to some of the remaining serious diseases. The increasing pressures for improved livestock production and plant food production have stressed the importance of economical and accelerated growth rates. Both of these areas have involved a greater interest in antiparasitics. The introduction of each new, and successful, antiparasitic has raised the standards for the introduction of the next. The standardizing and regulatory agencies are just as logically an outgrowth of these same pressures and competition.

The problems in the treatment of schistosomiasis with presently available toxic medication received considerable attention. Among the 60,000 Puerto Ricans in New York City, the exercise of clinical judgment and discrimination is constantly required; in the absence of exposure to reinfections it may be safer to let the infection run its course than to attempt therapy; however, serious sequela may develop in a limited number of untreated cases even in the absence of reinfection; the need for a better therapeutic agent is obvious. The lack of an effective agent against the lung fluke, *Paragonimus westermani*, was noted as well as the need for a better taeniacide and a better clonorchicide.

In the animal health field the 2 r's, resistance and residues, are problems to be faced with increasing frequency. Ignorance of parasitism remains a problem; education should help resolve this. Despite the progress made in

the study of the underlying principles of biological control, the control of parasites in the immediate foreseeable future will depend upon chemical agents. Phenothiazine has been, and still, remains the outstanding veterinary antiparasitic in the livestock field, despite the fact that it is expensive, bulky, produces red urine, and may discolor both wool and milk. Clear cut evidence of resistance has been limited to *Haemonchus contortus* in two flocks; other reports of resistance have been shown to be misinterpretations; however, this aspect will have to be watched carefully in the future. The effective use of phenothiazine against *H. contortus* has brought into sharp relief, the need for other agents against other nematode parasites.

In considering types and modes of antiparasitic attack, it appears significant that tissue invading forms and tissue invading stages have been attacked with chemical agents much less successfully than other stages. The systemic use of organophosphates to attack the tissue stages of the ox warbles is in sharp contrast to the failure to successfully attack the tissue migratory stages of any of the helminths with any of the therapeutically practical chemicals so far.

The history of the treatment of nematode infected plants and soils goes back over 80 years, but the organized attempts at widespread control have been developed largely during the last quarter of a century. The early use of hot water and the latter use of hot water and formalin both remain in use today in restricted areas. These procedures are most effective in the treatment of tubers on "fleshy seeds." The limitations on the temperatures which may be used without injuring the tubers precludes the desired 100% denematization of the tubers. Not all species of nematodes are killed by the same temperature. What is even more interesting is the evidence that strains of nematodes resistant to the hot water may be developing. The problems involved in the use of nematicides on seeds, tubers, and on soil with standing crops are fundamentally the same as the problems of using antiparasitics in man or domesticated animals, but the detailed problems may be quite different. In every case it is an attempt to eliminate, destroy, or prevent the occurrence of one form of biological life with a minimum, or preferably no, injury to another form of biological life. The treatment of fallow soil in advance of planting may be a departure from this principal.

We may have spent a disproportionate amount of time and discussion on one detail of the U. S. Food and Drug Administration regulations, but in the process some of the above scientific points were brought out. It was agreed that new, and presumably better antiparasitics should be forthcoming; the outlook will be improved by agents against parasites not significantly effected by the currently available antiparasitics. It was agreed that all of us concerned should focus even more sharply on the scientific data and the scientific interpretations of the data in the evaluation of these new agents as they appear. Gilbert F. Otto, Chairman.

FIFTIETH ANNIVERSARY BANQUET

This Banquet, held at the University of Maryland Dining Hall, was attended by 200 members and guests.

After the Banquet, President George W. Luttermoser turned the meeting over to Toastmaster Aurel O. Foster.

The University of Maryland was represented by Ronald Bamford, Dean of the Graduate School. Doctor Bamford not only made some very gratious welcoming remarks, but he also gave some personal sidelights on his early contact with the Society.

The Washington Academy of Sciences was represented by Dr. Heinz Specht, Secretary. Doctor Specht briefly reviewed the mutual goals and overlapping membership of the two organizations.

The American Society of Parasitologists was represented by Harold W. Brown, the President. Doctor Brown brought greetings from the Society and called attention to the fact that the Society originated within "Helm. Soc." and was formally organized by a Committee of its members appointed by "Helm. Soc." He also presented some personal reminiscences of "Helm. Soc." members and meetings during the middle twenties.

The Anniversary Award of the Helminthological Society of Washington was presented by Lloyd E. Rozeboom, Chairman of the Awards Committee. Doctor Rozeboom commented as follows: The sentiment for the establishment of a Helminthological Society of Washington Award apparently originated among the trustees of the Brayton H. Ransom Memorial Trust Fund, who also decided that the first presentation was to be made at the Fiftieth Anniversary banquet of the Society. Accordingly, an Awards Committee was appointed to make recommendations as to the nature and conditions of the award. Stated briefly, these are as follows:

The Award, known as the Anniversary Award, is to be presented not oftener than annually to a member of the Society in recognition of unusually meritorious service to the Society, or an outstanding contribution to science, or possibly some other activity which would advance the stature and influence of the Society. The award carries no monetary gratuity. It consists of a certificate, suitable for display if the recipient so desires, and of a public expression of commendation and honor from the Society.

The first recipient of the Anniversary Award was selected by the Awards Committee with due deliberation, but it must be confessed that the Committee arrived at a unanimous decision without difficulty. She entered Goucher College in 1917 and earned her B.A. degree in 1921, with a major in Biology. During the summer of 1920 she received a scholarship for study at the Woods Hole Marine Biological Laboratory. In 1921 she accepted an appointment as Junior Nematologist in the U. S. Department of Agriculture, and advanced through the ranks of Assistant and Associate Nematologist, and in 1954 was appointed Nematologist.

During the early part of her career in the U. S. Department of Agriculture, she worked in close cooperation with Dr. G. Steiner, and thus she embarked upon a scientific research program which has produced some three dozen publications on the plant nematodes. But, however significant these and other scientific contributions may be, we wish especially at this time to acknowledge the more than 25 years of service which she has given to the Society as Cor-

responding Secretary-Treasurer, and, unofficially, as Business Manager of the Proceedings. She took up these duties in 1935, and is still faithfully holding these offices. For 25 years the affairs of the Society have largely been in her hands. As anyone who has held office in any organization of this nature knows, small items of routine business pile up to a really formidable task. Our gratitude to one who has performed these services to the Society for such a long period of time, and has done so cheerfully and at such a high level of efficiency, is indeed profound.

It is, then, with a heartfelt expression of thanks that I hereby present the FIRST HELMINTHOLOGICAL SOCIETY AWARD to MISS EDNA MARIE BUHRER.

Edna M. Buhrer, Recording Secretary-Treasurer of the Society and Business Manager of its Proceedings, receiving the Helminthological Society Award (being presented by Lloyd E. Rozeboom).

THE BRAYTON H. RANSOM MEMORIAL TRUST FUND was represented by Gilbert F. Otto, Chairman of the Fund's Trustees. Doctor Otto briefly reviewed the life and professional career of Dr. Brayton Howard Ransom (Jour. Parasitol. 13: 1-15. 1926) and added some personal reminiscences of the oft repeated references to him and his work when the latter first joined "Helm. Soc." in the fall of 1927, two years after Doctor Ransom's untimely death. He briefly reviewed the origin and development of the Brayton H. Ransom Memorial Trust Fund which was initiated within a year of Doctor Ransom's death (Proc. Helm. Soc. Wash. 3: 84-87. 1936; 24: 145. 1957; this issue p. 250). Although there was an early interest in establishing an Award, part of the Fund earning was used to help establish, and is still used, to support the Proceedings of the Helminthological Society of Washington.

As announced in the Proceedings of the Helminthological Society of Washington (27: 208. 1960) the Trustees decided to make the first BRAYTON H. RANSOM MEMORIAL AWARD in 1960 and to make the presentation at this Fiftieth Anniversary Banquet.

Doctor Otto presented, on behalf of the Trustees, this first Award to JAMES HENRY TURNER in recognition of his scientific achievements and his conduct as a scientist. Doctor Turner was born in Stuart, Virginia, 13 June 1922 and reared in Maryland. Since we met on the campus of the University of Maryland, it seemed appropriate to remark that he received both his undergraduate and his graduate education at this Institution. His undergraduate education was interrupted by service in the U. S. Navy during World War II. He received the B.S. degree in 1947, the M.S. in 1952, and the Ph.D. in 1957. His scientific work included short periods in Japanese

James H. Turner, receiving the First Brayton H. Ransom Memorial Award (being presented by Gilbert F. Otto).

beetle control (1945) and in entomological preparations at the U. S. National Museum (1948), but his principal scientific work has been in the Beltsville Parasitological Laboratory of the Animal Disease and Parasite Research Division of the U. S. Department of Agriculture in Beltsville, Maryland. His career there began in 1948 with work on *Nematodirus spathiger* in lambs. In the twelve years since he began this work he has made effective contributions to our knowledge of the life cycles, natural history, pathology, treatment and control of the nematode parasites of sheep and goats. The results of these studies have been clearly and succinctly reported in some 30 scientific papers. He is currently acting leader of the work on the helminth parasites of sheep and goats.

It is the unanimous opinion of the Trustees that this young man, not yet 40 years old, has demonstrated the outstanding scientific acumen and qualities of leadership appropriate for the first recipient of the BRAYTON H. RANSOM MEMORIAL AWARD.

PARALOGUE AND PARASITE, the title of the principal address, by CHAUNCEY D. LEAKE, President of the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE brought the Fiftieth Anniversary Celebration to a close.

Chauncey D. Leake, the after dinner speaker on Paralogue and Parasite between Aurel O. Foster, the Toastmaster (left) and George W. Luttermoser, the President (right).

HISTORICAL RESUME

of the

Helminthological Society of Washington

The First Decade: 1910-1920

The Helminthological Society of Washington, known more informally as The Worm Club, had its origin on 8 October, 1910. On that date Dr. C. W. Stiles played host to four other scientists of more or less similar interests at the rooms of the Zoological Division of the Hygienic Laboratory, Public Health and Marine Hospital Service (forerunner of the National Institutes of Health), at 25th and E Streets, N.W., Washington, D. C. Dr. Stiles' guests were Messrs. Crawley, Garrison, Hall, and Pfender. Dr. Stiles was elected President and Mr. Hall Secretary of the newborn organization.

A rather interesting departure from the usual parliamentary procedure was initiated at this very first meeting. Instead of the President taking the chair, Dr. Garrison was named Chairman for the evening. At each meeting thereafter, a Chairman for the evening was appointed, a custom that continued until December, 1933. There is no record of the reason for this, but it seems possible that Dr. Stiles made it conditional on his accepting the presidency that he would not have to spend every meeting in the Chair. By thus rotating the chairman, the chore fell on each in turn, leaving everyone except the Chairman Pro-tem free to express himself from the floor.

The membership doubled at the second meeting, when Cobb, Foster, Leonard, Miller, and Ransom joined the original doughty band. At the fourth meeting, Goldberger was added. By this time, however, probably most of those scientists who were interested and available had been corralled. That additional members were hard to come by is evidenced by the fact that all but three of the meetings from the tenth (14 March, 1912) until the 47th (20 December, 1920) were held in the homes of the members. There is, of course, another possible reason for the comparatively small membership. The "Worm Clubbers" may have felt that they had a nice, congenial group and preferred to keep it that way. In any case, as late as 1922 there were only 34 active members, and these included the "Johns Hopkins gang," who joined up in 1919.

Although the active members may have been few, the Society very soon added much numerical strength and a great deal of prestige by electing foreign corresponding members. The first group of these consisted of Blanchard, Braun, Fuhrmann, Ijima, S. J. Johnston, Leiper, von Linstow, Looss, Luehe, Manson, Monticelli, Neumann, Nuttall, Parona, Perroncito, Pintner, Railliet, Shipley, and Zschokke. These were elected at the fifth meeting, and at the sixth meeting they remembered, and duly elected, Jagerskiold. We do not know whether these eminent gentlemen had any desire to join the Society, or, in fact, whether they had even heard of it. In addition to Jagerskiold, a group of American corresponding members were elected at the sixth meeting, viz.: Barker, Curtice, Kofoid, Linton, Nickerson, Pratt, Allen J. Smith, Swingle, Verrill, Ward, Wellman, Willetts, and Young.

The custom of adding corresponding members to the Society was continued at intervals. A sizable group was inducted at the thirteenth meeting (1913), the Americans being Bass, Darling, Herms, La Rue, Theobald Smith, and Strong, and the foreigners Brumpt, Cleland, Galli-Valerio, Gedoelst, Grassi, Henry, Huber, Janicki, T. H. Johnston, Loennberg, Mrazek, Nicoll, von Ratz,

and Wolffhuegel. To complete the list of corresponding members honored by the Society during the first ten years of its existence, Dr. Sweet was elected a foreign corresponding member at the twenty-third meeting (1914); and at the thirty-ninth meeting (1920), Yokogawa and Yoshida became foreign and Guberlet and G. A. MacCallum American corresponding members.

The practice of the Society was to meet several times a year, at the convenience of the members, and this custom was followed until the problems arising from the War, especially after the United States became a belligerent, forced a change. During the war years, official meetings were few and far between.

Several members of the Society served in the First World War, and it is possible that the 37th meeting may have been a special affair in their honor. A statement at the bottom of the minutes for the 36th meeting reads, "Note: No minutes will be published for the 37th meeting." However, the only member still living who was active at that time remembers nothing about this meeting and does not think that it was anything special. He says that there were several meetings between the 36th and 38th, and that probably no minutes were kept. However, the 37th was the only official meeting held between 26 Oct., 1917 and 18 Oct., 1919. In 1919, with the cessation of hostilities and the return to more or less normal conditions, the Society got back in business with more interest than ever. The frequency of the meetings was stepped up to a monthly schedule as one result of this renaissance.

The first three of these meetings (the 38th, 39th, and 40th) were held at the Zoological Division of the Hygienic Laboratory, as the 5th, 8th, and 9th had been. Thus the meetings, with few exceptions, were held at the homes of the various members. One of these exceptions was the banquet celebrating the tenth anniversary of the founding of the organization (44th meeting), and another was the first meeting held in Baltimore (41st), when on 20 March, 1920, the Johns Hopkins contingent was host to the Society. A third exception was probably the above-mentioned 37th meeting, details of which were not recorded.

There is much that is obscure regarding the conduct of the meetings of this period. Since most of them were held in the private homes of the members, it is probable that refreshments of some description were served. One wonders if the host had to foot the bill. And what about the bachelor members—did they get out of playing host?

The 43rd meeting, 29 May, 1920, was held at the home of Captain Daubney in Forest Glen, Maryland, and much correspondence regarding trolley car and train schedules took place. Forest Glen in 1920 was far out in the country, in practically a virgin wilderness.

The character of those early meetings was quite different from today. The Society had no constitution or by-laws and no dues were collected, although assessments were levied from time to time. There was no formal program, but every member was called on for contributions and the proceedings were mostly in the form of a round-table discussion. The individual members found nothing incompatible in being both eminent and hard-working scientists and men possessed of a goodly quantity of wit and a lively sense of humor. "The topics discussed at the meetings," to quote the history editor of the Fourth Annual Dinner, "ranged far and wide. To mention a few: Apparatus, sawdust, swine sanitation, excretory systems of flukes, relations of hookworms to snuff and soil and intelligence, more apparatus, coal-gas tar, fasciolospiniasis (sic), technic, nemas with legs, hundred-foot-long paper

nemas, the City of X, movies, L. R. S., CCl_4 nomenclature, amphids, our confreres, new parasites, new hosts, new records, nemas in soil, in water, in plants, in air, in history, sanitary law, anaphylaxis, endomixis, quarantine, commerce, laboratory accessories, more apparatus, and formulae—all strictly parasitological topics."

Until 1934, when the Society's very own journal was launched, publicity was a major and continuing problem. In 1910, there did not exist the bewildering array of journals that are published nowadays. A great deal of the work done by scientists employed by the U. S. Government was written up in the form of Circulars, Bulletins, and Technical Bulletins issued by Agriculture and other Departments. The only American journals that were reasonably suitable for the publication of the Proceedings of a society like the "Worm Club" were *Science* and the *Transactions of the American Microscopical Society,* and the latter at that time was exceedingly small. As it was the custom of *Science* to devote space to the publication of the minutes of various scientific societies, to *Science* the fledgling Helminthological Society naturally turned, and the minutes of the first 15 regular meetings duly appeared in its pages, but not without some bickering about the suitability of the material contained in those minutes.

The first of a series of disputations came when the report of the 7th meeting was submitted. On 6 January, 1912, the following letter was received from the editor of *Science,* Dr. J. McKeen Cattell.

"I fear that the enclosed abstract of the meeting of the Helminthological Society of Washington is too long and also somewhat too special in character for publication in *Science.* The secretaries of societies whose meetings are reported in *Science* are asked to limit the abstracts to 200 words, and while this is not always done, it is desirable that it should be done, and this is especially the case when the abstracts are of interest to a limited number of scientific men."

Dr. Cattell's letter produced an immediate and rather violent reaction. If *Science* didn't want to publish the Society's reports as submitted, they (the Society) would find some other outlet. Three courses of action seemed worth exploring. (1) Dr. Henry Ward, recently elected Corresponding Member, had been incubating some plans for a new journal, an American journal devoted to parasitology, and Dr. Stiles was appointed to write to him about this. (2) Secretary Hall was authorized to write Dr. Cattell about the possibility of buying space in *Science.* (3) Secretary Hall was further instructed to get some price quotations from publishers, in case the Society decided to issue its own journal.

Dr. Ward's plans for his new journal were not far enough advanced to be of any immediate help. Dr. Cattell, a man definitely not in the American business tradition, would not hear of accepting payment for space in his publication, but reiterated his plea for as much abridgement as possible. He accompanied his plea with an implied threat to discontinue publishing the Society's reports if they were not kept within bounds. The Cornman Printing Company of Carlisle, Pennsylvania, submitted a price for publishing that evidently was in excess of what the Society was willing (or perhaps able) to pay. Part of their letter, dated 29 January, 1912, may be of interest to amateurs of economics:

4 pgs 100 copies—$8.00. Extra copies, 35¢ per 100. Addt. pages—per page, $1.75. Extra copies, per 100, for publications having more than 4 pages, add 5¢ per page.

In any event, the will of the Society was to try to conform to the requirements of *Science* and to continue publishing therein.

The compromise lasted for about a year, for on 15 February, 1913, there came another yelp of anguish from Dr. Cattell, who was playing variations on the same old theme. It would seem that the Helminthological Society, with a proper appreciation of its importance to the Scientific World, was submitting abstracts of its meetings that were much more complete—and consequently more lengthy—than were those of other scientific societies that depended on *Science* for publicity.

Some sort of agreement was evidently reached once more, since the report of the 9th meeting (the latest report to meet with editorial criticism) was published in *Science,* as were the reports of the next six meetings. The "Worm Clubbers" were restive under *Science's* check-rein, however, as is evidenced by this extract from the minutes of the 10th meeting (18 December, 1913):

"Dr. Ransom discussed the new proposed publication of Dr. Ward's, and the Secretary was authorized to write Dr. Ward relative to this publication and the possible publication of the proceedings of the Society in it."

During the following nine months five more official meetings were held, a greatly accelerated pace. It seems possible that the Society hit upon the scheme of having more frequent meetings, with less to report on per meeting, as a means of getting fuller coverage in *Science* and at the same time keep Dr. Cattell calm. By the end of 1913, Dr. Ward's plans for his new journal were far enough along so that it promised a good alternative to *Science* as a medium for publicity. All the Society had to do, therefore, was to mollify Dr. Cattell for a few more months.

Volume 1, No. 1, of the Journal of Parasitology, managing editor Dr. Henry Ward, hit the news stands, figuratively speaking, in September, 1914, and included in it were the mere bare outlines of the proceedings of the 16th-20th meetings of the Helminthological Society of Washington. Thus in changing journals the Society got rid of one problem only to acquire several others.

On 10 December, 1915, Dr. Hall submitted the following letter to the Society, and at the same time provided evidence that scientists are, after all, subject to much the same foibles as ordinary people (Those who knew him testify that Dr. Hall had some pretty conspicuous foibles.): "In view of the friction and incompatability existing between the managing editor of the Journal of Parasitology and myself, I desire the Society to accept my resignation and to elect someone who can carry on the necessary correspondence with the Journal on a more friendly basis."

Three months later (3 March, 1916) both Dr. Stiles and Dr. Hall, who had served as President and Secretary, respectively, of the Society since its inception, handed in their resignations from these posts. The Society voted that in future officers were to be elected for a term of one year, with the proviso that they could succeed themselves. The new President was Dr. Cobb and the new Secretary, Dr. Crawley.

The new officers received their fiery baptism almost immediately. Included in the Proceedings of the Society submitted to the Journal of Parasitology for publication was a paper on non-parasitic nematodes authored by the new President, Dr. Cobb. Dr. Ward wrote to Dr. Ransom, 11 March, 1916, as follows: "I have a minor objection to running an article in society notes. I think the practice is open to even greater criticism when the article contains

a description of new forms. Will you not let me remove this part from the minutes and run it as a short separate paper under a heading you suggest and include a brief reference in the proceedings. If you agree, I should be glad to have you make the reference which should naturally be brief.

"Of course the articles are really a little outside our scope since the forms are not parasitic but in view of the circumstances, I will waive that difficulty, as the paper was presented before the Helminthological Society. Please let me have an immediate reply if possible."

Although Dr. Ward's objection was quite valid and not unreasonable, the "Worm Clubmen" would have none of it, and Secretary Crawley so informed Dr. Ward. Dr. Ward now shifted his ground and objected to the description of new species and genera appearing in society notes. He wrote a personal letter to Dr. Ransom on this subject, and Dr. Ransom was inclined to agree with him. They eventually decided to postpone publication of Dr. Cobb's paper until they could get things straightened out. Dr. Cobb inadvertently complicated the situation by becoming ill, so that his views could not be ascertained.

At the Society's meeting of 12 May, 1916, the matter of Ward's objections was gone into at considerable length and a format for future society notes was decided upon. This was submitted to Dr. Ward, and received qualified approval. In a letter dated 20th May he stated that he was sending the manuscript of the transactions of the March meeting to the printer. Nine days later, however, he wrote another letter, stating again that the Journal of Parasitology could not in the future publish papers on free-living forms.

Over a month now passed without any acrimonious correspondence, but the truce was broken on 9th July, when Dr. Ward once more took typewriter in hand to transmit some further thoughts on the subject of Dr. Cobb's manuscript. He said:

"I am much disappointed at the delay in the return of your proof. The manuscript is kept by the printer and not sent out with the articles. Our contract provides a minimum price, and does not allow of changes from copy we check in this way.

"Your proof certainly could be corrected in a few moments, and if there is any infelicity of expression that you desire to change, it can be marked. As this is proof-read in one of the best offices in the country, it is guaranteed to be like the manuscript. Please send by return mail and not hold the Journal's number longer.

"Regretting the necessity of urging your immediate action, I remain."

A hand-written letter from Dr. Crawley to Dr. Cobb, undated but obviously written right after he received the above, follows, in part.

"I enclose the proof of the Proceedings of the 28th and 29th meetings of the Society. This was sent me without the Mss., and when I wrote Dr. Ward for it, I received the rather objectionable letter which I enclose. I sent a reply to Ward, stating that the whole thing was referred to you. My reason for taking this last action being that your contributions in these proceedings are by far the most important, and also those needing the most careful proof-reading. I am sorry to put this trouble on you, but I did not care to attempt proof-reading without the mss. from which the paper was set up.

"Strictly between ourselves, I wish to say that dealing with Ward requires more Christian patience than I've got, and if the Society is to continue its

dealings with the Journal of Parasitology I desire to be excused from that part of the secretarial duties . . . I suggest that you go over your part of the proof, and send it back to Ward yourself, since this seems to me the best way out of the snare. . . . I am sorry for the whole affair, but don't see what else we could have done."

The following letter from Dr. Cobb to Dr. Ward seems to raise some doubts as to the capabilities of "one of the best offices in the country" as far as proof-reading ability is concerned.

"I returned to Washington last evening; and Dr. Crawley handed me the proof the first thing this morning, and I have corrected it immediately and am returning it herewith, together with manuscript.

"I have inserted titles which will do away with indexing difficulties . . . The illustrations were put in the wrong place. I have cut the galley and placed the illustrations where they should go . . . It was so placed in the manuscript. I am sorry the printer has misunderstood the dichotomous key. As it stands it is likely to be puzzling to the reader, and my wish in the matter is that it be set as originally written. The key, as originally written, is strictly dichotomous, properly indented, and with leading words and capital letters arranged in standard way, and was designed to fit type of the Journal; and I believe in no case will the subdivisions of the key overrun a single line. It hence gives the reader very plain sailing.

"The marks adjacent to the letter 'f', which stands for female, are dashes and single quotation marks. The dashes are correctly printed in the proof, but the quotation marks have been set as acute accents."

The whole tempest in a test tube so upset the Society that they went a-seeking bids from printers again. However, just as was the case four years earlier, nothing came of this except another printer's quotation to file away. The price of printing, they found, had gone up, and especially the price of paper, the result of the war currently being waged over a good part of the world.

The war, or something, seemed to have one by-product on the credit side. There is no record of any internecine strife on account of editorial differences again until 1920, and that comes under the Second Decade.

The first decade in the life of the Helminthological Society of Washington may be said to have culminated in the (First Annual) Banquet held at the Hotel Continental in Washington, 20 June, 1920, in lieu of the 44th meeting and in celebration of the tenth anniversary of the founding of the Society. This affair was publicized as being strictly informal and of a frivolous nature. It cost $3.00 per person and the waiter received a $2.00 tip. (It is to be hoped that he did not spend it foolishly.)

Some of the blithe spirits among the junior members of the Society seized the opportunity that the banquet presented to have a bit of fun at the expense of their older, more staid colleagues. An eye-witness report has it that the ragging was not an instantaneous hit, but that, since no favorites were played, the objects of the lampoons eventually entered into the spirit of the thing. Limericks were used as place cards and fictitious papers were presented, attributed to suitable members. An award by the "Hellological Society of Washington" was made to Dr. Stiles, the first President, "Summa cum Maude of Privy Counsilor and Closet Naturalist." Various members made individual contributions to the fun. Young Ben Schwartz, who had celebrated his 30th birthday the previous year, gave a reading of "A Lay of Modern Parasitology" (with apologies to Macauley), presumably written

by this precocious young man. Something called "The Parasitologists' Song" was rendered by Chapin (26), Daubney (29), and Hall (39). These men were not children, but were still mere lads as age is reckoned among scientists. Daubney was a "distinguished foreign visitor" rather than a member of the Society, but he seems to have entered pretty thoroughly into the spirit of fun, probably at the instigation of Hall. The pair of them further edified the revelers with a song to "Haemonchus," written as a parody on "Cock Robin." On the whole, the slightly premature end of the first ten years of the Society seems to have been quite a gala affair that was enjoyed by almost everybody.

The Second Decade: 1920-1930

Two more meetings, the 45th and 46th, were held in the homes of members. The 47th meeting (20 November, 1920) was held at Johns Hopkins University, the Baltimore contingent acting as hosts. From then through 1934, the November meeting was at Johns Hopkins. The 48th meeting (20 December, 1920) found the Society assembled in Dr. Bartsch's room at George Washington University. The change was necessary because attendance had grown so large that the group could no longer be accommodated in private residences. There is no reference to wifely pressure in the minutes, but the probability is that the distaff side had a voice in the decision to find a regular meeting place. George Washington University remained the Society's home until 1923. The 17th day of February, 1923, found it assembled in rooms in the East Wing of the U. S. Department of Agriculture. Quarters were again shifted, 18 May, 1929, this time to the U. S. National Museum.

The publication problems of the first decade held over into the second, but with diminished rancor. On 5 August, 1920, Dr. Ward requested as much condensation as possible. His reason was the greatly increased cost of printing. The Society felt that he had a valid point, and made no difficulty about cooperating.

The next evidence of unhappiness about publication of the Society's Proceedings is found in the minutes of the 79th meeting, 18 October, 1924. The cause of complaint was the delay in printing. According to the minutes of that meeting, the last meeting for which Proceedings had been published was the one held 15 March, 1924. They decided to approach the Washington Academy of Sciences. The latter's reply, in several hundred well-chosen words, boiled down to No! So relations with the Journal of Parasitology were continued, and remained on at least a tolerable basis for several years.

Evidence that the Society was on the way to becoming a Force in the world of science is provided in a letter from Dr. Allee, Secretary of the American Zoological Society, read at the 47th meeting (20 November, 1920), in which the idea of forming a division or section of Parasitology in the Zoological Society was broached. The "Worm Club" was favorably disposed toward this idea, with certain reservations, but nothing came of it.

The Second Annual Banquet had originally been scheduled for 29 May, 1921, but in view of the fact that Dr. Cort and others were to leave for Trinidad around the 1st May the date was advanced to 9 April. This dinner, like the first one, was held at the Hotel Continental, and cost the members $3.60 apiece, which covered the cost of the two guests present, Drs. Henry Ward and L. O. Howard. They were rather more liberal with the waiter this time, giving him $8.00. It seems to have been quite a feed, as the menu testifies:

Blue Points or Little Neck Clams
Celery—Olives—Radishes
Mock Turtle Soup
Broiled Shad Maitre d'Hotel
Julienne Potatoes
Roast Turkey, Cranberry Sauce
Puree of Potatoes—Creamed Cauliflower
Grapefruit Salad
French Dressing
Ice Cream Assorted Cakes
Demi Tasse

The second banquet continued the fun and games motif established at the first one. It was particularly memorable for two things: first, the uplifting spectacle of Messrs. Hall and Crawley being polite to Ward, their guest; and second, the write-up it got in the Washington Herald, in that newspaper's Scientific Notes and Comments department. Among other things, the Herald printed "The Song of the Helminthologists," presumably the work of Hall:

> Sing a song of thymol, a pocket full of hooks,
> A patient full of all the worms you read about in books.
> When the man was opened they found the worms had fled;
> The doctors, they were satisfied, the patient, he was dead.

At the 53rd meeting (14 May, 1921) they voted to establish an active membership list consisting of those who agreed to pay $1.00 a year dues. At the next meeting, the 54th (21 October, 1921) a committee was appointed to deal with corresponding members, both native and foreign.

At the 58th meeting (18 February, 1922) a committee, headed by Dr. Stiles, was appointed to draw up a "Professional Code of Ethics." The fruits of their labor were presented at the 61st meeting (22 April), and were adopted with some amendments.* The core of this code had to do with the proper behavior of Society members toward one another, with especial emphasis on the rights of individuals in respect to publication of the results of investigations. An interesting piece of research might be done on the reasons why an organization such as the Helminthological Society felt such a Code of Ethics necessary.

The Third Annual Banquet was held on 1 April, 1922, this time at the Hotel Lafayette. The same spirit of fun that had featured the previous banquets prevailed. Guests included Dr. Louis Shapiro of the International Health Board, Dr. Barres Barretto of the Institute of Oswaldo Cruz, and G. Martinaglia from Johannesburg. The high spot of the banquet was the presentation of the Steel Memorial Medal to Dr. Hassall.

At the 61st meeting (22 April, 1922) a letter from Dr. A. J. Goldfarb, of the American Association for the Advancement of Science, was read. The Society was asked to cooperate with the AAAS as a parasitological adjunct of Section N (Medical Sciences), in its future programs. The Society was favorably disposed, but nothing came of it.

The Helminthological Society voted at its 65th meeting (20 January, 1923) to apply for membership in the Washington Academy of Sciences. The application was duly made, and the Washington Academy informed the Society, in a letter dated 6 March, 1923, that it had been accepted.

*Journal of Parasitology 9: 46-47. 1922.

The Fourth Annual Banquet was held 21 April, 1923, this time at the Ebbitt Hotel. It was conducted in the same spirit of fun as the previous ones, and was featured by the publishing of the "Helminthological Herald and Parasitological Picayune."

The year 1924 was notable for two things, one positive and the other negative. To dispose of the negative in a very few words, there was no Annual Banquet that year. The positive consisted in the formation of the American Society of Parasitologists.

The role of the Helminthological Society of Washington in the founding of the American Society of Parasitologists begins with a letter from Dr. Hegner of Johns Hopkins to Dr. Ransom of the Bureau of Animal Industry, dated 5 January, 1924. Dr. Hegner wanted to get Dr. Ransom's personal reaction to the idea of forming such an organization, with the Helminthological Society as its nucleus, and he presented several arguments in its favor. Dr. Ransom was receptive to the idea, provided that the Helminthological Society did not lose its identity in the process.

The subject was brought up by Dr. Hegner at the next meeting, the 73rd (19 January). The American Corresponding Members were queried, and a favorable reply was received from the great majority. At the 80th meeting (23 November) the Society passed a motion to call a meeting of parasitologists while the American Association for the Advancement of Science was holding its annual meeting in Washington during Christmas week. They also voted to donate $10 to help defray the expenses of the AAAS! The committee appointed to take care of the plans for the proposed new organization consisted of Dr. Hegner, Chairman, Miss Cram, Secretary, and Drs. Ransom and Cort. (In passing, it appears that Miss Cram must have devoted most of her spare time to what amounted to a second career as secretary to this, that, and the other society and committee.)

The result of all this was that the American Society of Parasitologists, after a gestation period of approximately a year, was born on Tuesday, 30 December, 1924, at Central (now Cardoza) High School, Washington, D. C. Dr. George La Rue officiated at the birth, and Dr. Cort took notes. The three officers and six of the eight members of the Council at Large at the outset of the new Society's career were either active or corresponding members of the Helminthological Society.

Dr. Brayton H. Ransom died 17 September, 1925. At the October meeting of the Society (17 October) a committee was formed to consider plans for some sort of memorial to Dr. Ransom. Dr. (Miss) Cram was committee secretary. Numerous suggestions as to the nature of the memorial were received. Meanwhile, the committee collected contributions for a Ransom Memorial Fund. These contributions had reached a total of $930 cash in hand by December, 1927. At the Society's 117th meeting (19 January 1929), a committee was elected whose function was to nominate a committee of trustees for the Fund. At the 118th meeting (16 February) Dr. Cort, Dr. Schwartz, and Mr. Christie were elected "to serve as a committee of trustees" of the Ransom Memorial Fund.

The Seventh (and last) Annual Banquet, celebrating the 60th birthday of Dr. Stiles, was held 21 May, 1927. Two years later, 11 May, 1929, a special dinner was held, honoring Dr. Cobb on his 70th birthday. The second decade in the life of the Helminthological Society of Washington was celebrated with another Banquet, held at the Hotel Continental, 20 October, 1930.

The Third Decade: 1930-1940

The U. S. National Museum, where the Society had started holding its meetings in the spring of 1929, continued to be "home" throughout the '30's. The annual November treks to Baltimore were made through 1934, after which they were discontinued until 1940. The 165th meeting (20 October, 1934) was held at the Zoological Division, U.S.D.A., Beltsville, to dedicate the new laboratory.

The smoldering antagonism between the Society and the Journal of Parasitology, which had remained quiescent for an unprecedented five and a half years, broke into flames again in the spring of 1931. The Society's side of the disagreement is detailed in the minutes of the 139th meeting (16 May, 1931):

"The Secretary read a letter from Dr. Henry Ward, Editor, Journal of Parasitology, returning certain notes presented by Dr. Cobb on free-living nematodes and stating that such notes by previous agreement could not be accepted for publication in the Journal of Parasitology as part of the Proceedings of the Society. Dr. Ward also stated that he was deleting from the manuscript of the proceedings certain resolutions adopted by the Society on the deaths of Prof. Railliet and Prof. Ohdner. Dr. Hall's reply to this letter was also read. Dr. Schwartz stated that while he was secretary of the Society, an agreement was entered into whereby notes on free-living nematodes would not be presented for publication in the proceedings of the Society. Dr. Stiles moved that the Secretary telegraph Dr. Ward to return all manuscripts of the proceedings. Dr. Bartsch suggested that the proceedings might be published in the Journal of the Washington Academy of Sciences. Dr. Cort urged that the Society carefully consider the matter before taking action and that a committee be appointed to negotiate with Dr. Ward. Dr. Schwartz moved that the Chair appoint a committee of three to take up with Dr. Ward the questions in disagreement and, if necessary, to offer Dr. Ward an honorarium in connection with the publication of the Proceedings of the Society in the Journal of Parasitology. . . . The Chair appointed Doctors Cort, Schwartz, and Cobb."

The committee worked out a temporary truce with Dr. Ward. At the 141st meeting (16 October, 1931), Dr. Cort was named a committee of one to edit the Proceedings before submitting them to the Journal of Parasitology. Dr. Ward visited Washington in the latter part of the month, and Drs. Cobb and Schwartz, acting for the Society, had a conference with him. The outcome of the conference was reported as successful.

A year of peaceful relations followed. In September, 1932, Dr. Ward turned the Journal of Parasitology over to the American Society of Parasitologists, and Dr. Cort became the new editor of the Journal. Since he was also the editor of the Society's Proceedings, this should have been an ideal arrangement, and in fact for several months relations were excellent. How long Dr. Cort continued in his dual editorial capacity is not clear, but by May, 1933, he had definitely gone over to the enemy. It was the same old story: the Society's Proceedings (a) were too long, (b) contained material unsuitable for publication in this form, and (c) were of trifling and inconsequential content.

There is no mention of anyone moving to drum Dr. Cort out of the Society, and in fact he had some support for his position, notably from Dr. Schwartz. Dr. Steiner took up the cudgels for the Society and replied to both Cort and

Schwartz in a most eloquent and well-reasoned letter directed to Dr. Schwartz, and reproduced in part here.

"We received Dr. Cort's letter and your comments. . . . We certainly agree that something must be done in regard to our Proceedings to help the editor of the Journal of Parasitology. If I understand your note correctly, you figure the matter might be taken care of by curtailing the Proceedings and establishing certain rules excluding matters covering certain fields of helminthology. Would this not be very unwise and involve a complete change of our policies? The Helminthological Society of Washington has grown to what it is just by having very few rules and in the past the Society has covered not only the fields covered in its name, but very often has considered other fields, and, we think, much to its good.

"The scope of the Helminthological Society covers all the fields of helminthology, including taxonomy and nomenclature. These latter have even been main parts in its activities. We believe it would be unwise to rule out such things because:

1. We have a number of members whose official duties cover these fields, and in ruling out such matters it would practically amount to placing these members on the second class list.

2. We doubt that the Society would ever decide to determine, if such a thing could be done, what is important and what unimportant in the fields of research covered by its name. Let us stay away from snobbery, which of late years has come up in quite a few fields of our biological sciences.

"Our Society has been a free society in its conception and in its activities. Of course, in our time of Hitlerism, Stalinism, Mussolinism, Kemalism, or whatever it may be called, scientific activities may also be infringed on by such tendencies of Gleichschaltung. I do not think that this would help; I rather believe it would mean the breaking up of our now flourishing society.

"A scientific fact is a fact; it may look unimportant today, very important tomorrow. We have a number of young members. They need encouragement. Let us give it to them by proper consideration of their work and results. Let us be liberal to all members of the Society by such consideration and by accepting as equal the facts uncovered by all of them. Let the Proceedings be a full record of our very varied and wide research life. I doubt the advisability of charging the editor with the task of judging a note as to its verbosity. It would very often place the editor in an awkward position, especially if such verbosity concerns his superior, a case far from hypothetical. Neither does it seem advisable to restrict the number of papers to be given by one member at one meeting. Results of investigation shall be given when they are ripe. A member may have no note for a dozen meetings and suddenly find himself in a position to present half a dozen valuable contributions at one single meeting. Why not? Are we going to interfere?"

Since the publication question seemed to have boiled down to a choice of either subsidizing the Journal of Parasitology or launching a separate journal, either of which was going to cost money, Dr. Chitwood presented some cost figures at the 157th meeting (21 October, 1933). The matter of separate publication was discussed very fully at this and the next (18 November) meeting. A poll of the Society's membership was conducted, and at the 159th meeting (16 December, 1933) the Society voted for independent publication, on a motion by Dr. Dikmans. The Proceedings of the Helminthological Society of Washington, vol. 1, no. 1, was issued in April, 1934, writing *finis* to a source of trouble that had plagued the Society almost from its inception.

In the course of the discussion that took place at the 159th meeting in regard to the Society publishing its own journal, Dr. Price announced that the Society, legally speaking, did not exist. In order to give it legal status, action should be taken, he thought, to provide it with a constitution. If it had no power to make a legal contract, it could not very well publish a journal. The point was well taken, and the committee on publication of the proceedings appointed a subcommittee, composed of Drs. Price and Dikmans, to formulate a constitution. The subcommittee did its job, and at the 160th meeting (20 January, 1934) Dr. Chitwood presented the fruits of its labor to the Society. After the non-believers had been afforded an opportunity to speak their pieces, the constitution was adopted.

The adoption of a constitution was really quite an historic moment in "Worm Club" history. It was the third step in a progression that did away with the free-and-easy atmosphere that had characterized the earlier years. The first of these steps had been taken at the 142nd meeting (20 November, 1931), when a definite membership policy was adopted. Membership was divided into three classes: active, foreign, and honorary. Honorary membership was to be conferred much as a college or university confers an honorary degree. Foreign members would henceforth have to apply for and be elected to membership. Active members would in the future have to pay for the privilege at the rate of $2.00 a year ($1.00 if they didn't get a copy of the Proceedings), and if they allowed themselves to become two years delinquent they could be dropped from the organization without notice. In 1934 dues were raised to $5.00, deemed necessary in order to handle publication expenses. The second step was taken at the 159th meeting—they were certainly a busy group at that particular meeting—when the Society voted to discontinue the Chairman of the Evening custom and require the President to conduct all of every meeting, both the scientific program and the business meeting. This last rule was relaxed as of the 230th meeting (18 November, 1942), to the extent that when the Society assembled somewhere other than its regular meeting place a Chairman, representing the host institution, once again presided over the scientific program.

At the 141st meeting (16 October, 1931) Dr. Schwartz suggested that some action be taken by the Society to arrange for having the Ransom Memorial Fund make an award to someone for outstanding work in parasitology. The Society acted with great promptitude and named Drs. Cobb, Cort, and Hall a committee to select a suitable recipient. The committee acted with equal dispatch, recommending at the very next meeting that some deserving graduate student be named to receive the award, but that a year's time be given the committee to prepare policy and select the recipient. Dr. Cort made a motion that the present committee be discharged and a new one named to do this, but he was outmaneuvered: The Society carried Dr. Ewing's motion to continue "the present committee."

Six months later (148th meeting, 21 May, 1932) a report from the committee was called for. Dr. Hall reported that the committee had asked schools to submit papers for consideration.

One year after the appointment of the committee, at the 149th meeting (15 October, 1932), no recipient had yet been picked. Dr. Schwartz then proposed that the decision be postponed for a year, and Dr. Steiner succeeded Dr. Cobb on the committee.

At the 155th meeting (15 April, 1933), Dr. Schwartz asked whether an award was to be made that year. Dr. Steiner reported for the committee

that no action had been taken.

We next hear of the Ransom Memorial Fund at the 169th meeting (16 February, 1935). Dr. Hall was asked for information about the status of the Fund. He reported no action, but made two recommendations. He suggested that the money be invested in U. S. Government bonds, which paid a higher rate of interest that the savings account in which it was presently kept. His second suggestion was that a new committee be formed, with Dr. Steiner as chairman.

Three meetings later (172nd, 18 May), Dr. Steiner raised the question of the legal status of the Society *in re* the Fund.

At the 180th meeting (16 May, 1936), a board of Trustees for the Fund, consisting of Drs. Cram, Dikmans, Otto, Price, and Steiner, was created as a body completely independent of the Society. For that reason, the story of the Ransom Memorial Fund as an integral part of the Society ends here, ten years and seven months after the naming of the first committee and 24 years and 5 months before the naming of the first award.

Turning to the lighter side, we find that the 1930's provided fewer festive occasions than did the previous decades. Dr. Stiles entertained the Society at his home in May, 1931, and on 18th July, 1938, Dr. Bartsch was host at a special meeting in his large rock garden, a show place in which he took a justifiable pride. A special dinner was held at the Dodge Hotel, 5 February, 1932, the occasion being the 70th birthday of Dr. Hassall, and the Society gave a banquet to celebrate the 200th meeting, 14 January, 1939, at the Hotel Continental. The first of a series of Annual Picnics was held at the Log Lodge, Agricultural Research Center, Beltsville, 18th May, 1940. There is nothing in the minutes to indicate that this was in celebration of the 30th Anniversary, but it could well have been.

RECENT TIMES: 1940-1960

Happy is the nation without a history, wrote the 18th century Italian, Beccaria. If what is good for nations is good for scientific societies, the "Worm Club" must have been a happy group for the last twenty years. The most memorable things that have happened during this entire period have been the revision of the constitution and the visits to various institutions for the purpose of holding meetings.

During World War II the rationing of gasoline created a transportation problem, and in order to equate the expenditure of precious coupons the Society voted to hold half its meetings in different laboratories. The first of these, the 230th, was at the Zoological Division Laboratory, Beltsville, 18 November, 1942. This developed into an annual event, all November meetings since 1942 having been held as guests of the Zoological Division (or its successor, Animal Parasite Investigations) with the exception of 1945, when the Nematology Division, Plant Industry Station, played host.

The Baltimore meetings, which had been discontinued after 1940, were started again in December, 1947. Starting with 1948, the Society has met in Baltimore every April, except 1954 and 1955, when the month was May. The meeting of the 21 May, 1954, was a rather special occasion, being the dedication of the Cort Library, School of Hygiene and Public Health, Johns Hopkins University.

The Catholic University of America provided a place for the 232nd meeting (13 January, 1943). Since then, 31 meetings have been held at Catholic

U., which succeeded the National Museum as the regular meeting place for the Society as of December, 1948.

The National Institutes of Health have entertained the Society at all March meetings, starting in 1943, except for the years 1944 and 1950, when the January and February meetings, respectively, were held there.

Georgetown University rallied round in February, 1944, and again in April of both 1954 and 1955. The Army Medical Center played host in February of 1947, March of 1950, February of 1954, January, February, and December of 1955, and January of 1957. Not to be outdone by the Army, the Navy Medical Center took over the hosting in February of 1953, December of 1954, February of 1957, and January of 1959. Meetings have been held annually at the University of Maryland since 1954 (except 1957), January being the month in 1954 and 1958, February the other years. To complete the roll call of host institutions, Howard University took over in December of 1956, 1957, and 1959. The 1959 meeting was held jointly with the Howard University Society of Sigma Xi and the Washington Society of Tropical Medicine, and the speaker of the evening was Dr. Manson-Bahr.

The picnic held at the Log Lodge at Beltsville in the spring of 1940 evidently went over pretty big, picnics having been held every year since then, usually in May but occasionally in June. The ARC Log Lodge has been the site of most of them, with the Horticulture Station Log Lodge serving from 1941 to 1945, and again in 1948, 1949, and 1950. One Annual Picnic, 16 June, 1951, the 300th meeting, was held at Dr. Bartsch's home, Lebanon, in Lorton, Virginia.

In addition to the Annual Picnics, there were two Banquets or Special Dinners over this 20-year span. The first of these, 21 March, 1945, at the 400 Club, was a Special Dinner to celebrate the 250th meeting of the Society, and among those present by special invitation were Lt. Com. Elon E. Byrd, Col. Justin M. Andrews, and Lt. Col. Hardy Kemp, who gave accounts of special investigations done by them during the war. At Lee House, 11 October, 1950, a Banquet was held to commemorate the Society's 40th Anniversary.

At the 236th meeting (15 May, 1943) a committee, consisting of Dr. Steiner, Miss Buhrer, and Drs. Dikmans, Foster, and Olivier, was appointed to revise the constitution. It was felt by some that a stronger constitution was needed, containing provisions for a central executive committee and regularizing certain proceedings. It was felt by others (and quite strongly) that the present constitution was perfectly satisfactory, assuming that any constitution at all was needed. At the 238th meeting Dr. Dikmans replaced Dr. Steiner as chairman and Mr. Lucker took Dr. Olivier's place on the committee. Eyewitnesses recollect that the debate, which lasted through most of 1944, was spirited. One of the more thought-provoking subjects seems to have been a proposal by Drs. Cort and Otto to change the Society's name to a more general one. There was a feeling that members working in fields other than helminthology were being slighted. The members were polled and the majority voted in favor of retaining the time-honored name. At the November, 1945, meeting the revised constitution was finally adopted and turned over to the editor, Dr. Christie, for publication in the forthcoming Proceedings.

An event worthy of at least passing notice, in the light of recent national economy, occurred in 1950: dues were reduced from $5 to $4 a year (and they have not since been increased). This is especially noteworthy in view of the financial struggles of the early days of independent publication, when

contributors to the Proceedings had to furnish cash at so much per page as well as the material itself. The journal not only has steadily increased in size, but has gotten on its economic feet while so doing. Nowadays member contributors are not required to pay anything for publication unless they have a paper that is either inordinately long or contains an unusually large number of tables or illustrations.

At the 360th meeting (21 January, 1959) the Helminthological Society of Washington voted to apply for affiliation with the American Society of Parasitologists, provided that the ASP change its by-laws, which required that all officers of an affiliate be members of the ASP. The application, with proviso, was duly forwarded to the ASP, 2 February, 1959. The ASP in due time voted to adopt an amendment to its by-laws in conformity with the wishes of the Helminthological Society, and formal acceptance of the latter as an affiliate of the former was comunicated in a letter dated 17 December, 1959. Thus a full circle was completed and a sort of Nirvana achieved: the Helminthological Society, which created the American Society of Parasitologists, was accepted by the American Society of Parasitologists as an affiliate.

The Helminthological Society has come a long way since the original five gathered on that fall evening in 1910. As of January, 1960, there were 379 members, located in 42 states, the District of Columbia, the Canal Zone, Puerto Rico, and 27 foreign countries. The latest (September, 1960) figures list 1 honorary, 5 life, and 388 resident and non-resident members. The great increase in interest in the Society is pointed up by the fact that in the 10-year period 1950-1960 membership more than tripled.

To climax a Half-Century of Progress, the upcoming day-long program and Banquet at the University of Maryland was held on 8 October, 1960.

Charter Members

Chambers, W. E. d. Washington, 1920. Artist, engraver, technician. Mr. Chambers became acquainted with Dr. N. A. Cobb in Australia. He worked with him there and accompanied him to Hawaii and from there to Washington. Dr. Cobb's papers were illustrated by Mr. Chambers, whose drawings and engravings were wonderfully clear and detailed.

Cobb, Nathan Augustus. b. Spencer, Mass., 30 June, 1859; d. 4 June, 1932. B.S., Worcester Polytechnic Inst., 1881; teacher, Williston Seminary, 1881-1887; Ph.D., Univ. Jena, 1888; Naples Zoological Sta. (appointed by British Assoc. for Adv. Sci.), 1 year; locum tenans for Prof. Haswell, Univ. Sidney, 1890; Pathologist, Dept. Agr. N.S.W., 1891-97; Agr. Comm., N.S.W., 1898-1901; Pathologist, Dept. Agr. N.S.W., 1901-4; Director of Division of Physiology and Pathology, Hawaiian Sugar Planters Exp. Sta., 1905-7; Agr. Technologist, U.S.D.A., 1907; remained in Dept. Agr., eventually attaining rank of Principal Nematologist, and was for some years Acting Asst. Chief, Bur. Pl. Ind. At various times served as President of Am. Micr. Soc., Am. Soc. Par., Wash. Acad. Sci., and Helm. Soc. Washington.

Dr. Cobb, who became a world-famous nematologist, was a striking combination of zoologist and technician, combining the technical and mechanical ability generally attributed to the Yankee with the scientific tradition of the German school. He was equally proficient at either constructing or using a camera or a microscope. Dr. Cobb's laboratory was a show place, where visitors were impressed by his careful procedures and elaborate and efficient apparatus. The microscopes that he used for fine work were placed on deeply sealed supports to prevent vibration. Microscopes and other appara-

tus used for less critical work were placed on revolving tables, so that they could be brought to the worker who presently needed them, thus turning his laboratory into a sort of scientific assembly line. Personally, Dr. Cobb combined frankness with courtesy, an unshaken dignity with a keen sense of humor. Along with his other talents, he had unusual histrionic ability and wrote delightful verse.

CRAWLEY, HOWARD. b. Philadelphia, 4 June, 1869; d. 27 May, 1929. B.S., Univ. Pennsylvania, 1897; M.S., Harvard Unvi., 1901. Joined Bur. Animal Ind., 1908, as protozoologist; resigned, 1919; appointed parasitologist, Penna. Bur. Animal Ind., 1919. Mr. Crawley's special field was the Sporozoa, with particular emphasis on the Gregarinida.

FOSTER, WINTHROP DAVENPORT. b. Jersey City, 28 Dec., 1880; d. Washington, 1918. B.A., Williams College, 1904; high school biology instructor, Assumption, Illinois, 1904-5; clerk, Census Bureau, 1908-10; Junior Zoologist, Zool. Div., Bur. Animal Ind., 1910-18. Mr. Foster received his M.A. from Williams College in 1912. He studied veterinary medicine at George Washington University. His work in parasitology was principally with the parasites of swine.

GARRISON, PHILIP EUGENE. b. 1877. Probably M.D. (listed by Index-Catalog as a doctor in New York City). Dr. Garrison was connected with the U. S. Naval Medical School at the time he was active in the Helminthological Society. His main interest was parasites of man in the Philippines.

GOLDBERGER, JOSEPH. b. Gereld, Hungary, 15 July, 1874; d. 17 Jan., 1929. M.D., Bellevue Hospital Medical College, 1895. Asst. Surgeon, Past Asst. Surgeon, Surgeon, U. S. Public Health Service, 1899-1929. Dr. Goldberger was a bacteriologist, epidemiologist, and helminthologist, and he contributed epoch-making studies on the cause, treatment, and prevention of pellagra. He worked with Dr. C. W. Stiles on helminths.

GRAYBILL, HENRY WEBSTER. b. Hamilton Co., Neb., 18 June, 1875; d. 1938. B.S. 1900, M.A. 1902, Univ. Nebraska; High school biology teacher, 1902-1905; zoologist, Bur. Animal Ind., 1905; D.V.M., George Washington Univ., 1911; Professor of Zoology and Parasitology, George Washington Univ., 1911-14; resigned from Dept. Agr., 1914, to become Animal Pathologist and Veterinarian, American Smelting and Refining Co.; joined Dept. Animal Pathology, Rockefeller Inst., 1916; later served as pathologist, California State Dept. Agr.

HALL, MAURICE CROWTHER. b. Golden, Colo., 15 July, 1881; d. 1938. B.S., Colorado College, 1905; M.A., Univ. Nebraska, 1906; taught high school biology and chemistry, 1906-7; Junior Zoologist, U.S.D.A.; 1907; Ph.D., George Washington Univ., 1915; D.V.M., George Washington Univ., 1916; parasitologist, Parke Davis & Co., 1916-18; Senior Zoologist, Bur. Animal Ind., 1919, eventually becoming Chief, Zool. Div.; Chief, Zool. Div., Nat. Inst. Health, 193-. In his spare time, Dr. Hall functioned as Professor of Zoology and Parasitology, George Washington Univ. (1913-16); Special Lecturer Parasitology, Detroit Coll. Med., 1917-18; and Lieut. Vet. Corps, 1918-19. Dr. Hall's chief interests were parasites of mammals, insecticides, and anthelmintics.

HASSALL, ALBERT. b. Woolwich, Kent, 11 Feb., 1862; d. 18 Sept., 1942. M.R.C.V.S., London, 1886. Joined Bur. Animal Ind., 1887; retired as Asst.

Chief, Zool. Div., Bur. Animal Ind., 1932, but continued to serve as Collaborator. Dr. Hassall's chief interest was the Index-Catalogue of Medical and Veterinary Literature, perhaps the greatest contribution to parasitological literature. In recognition of this, he was awarded the Steele Memorial Medal by the Royal College of Veterinary Surgeons in 1922.

Leonard, George F. Published with Dr. Stiles in Public Health Reports; carried out investigations on parasitic diseases at the Marine Hospital, Wilmington, N. C., circa 1910.

Pfender, Charles A. b. 1878; d. 1926. M.D. Dr. Pfender attended meetings regularly during the earlier years of the extistence of the Society, and made frequent contributions to the Proceedings. His professional interests gradually became centered in his specialty, roentgenology, in which field he acquired a reputation. He kept his interest in the Society, however, and whenever present at meetings he would usually present a note on some phase of parasitology encountered in his practice.

Ransom, Brayton Howard. b. Missouri Valley, Iowa, 24 March, 1879; d. Washington 17 Sept., 1925. B.S. 1899, M.A. 1900, Univ. Nebraska; Fellow in Zoology, Univ. of Missouri, 1900-1; Fellow in Zoology, Univ. of Nebraska, 1901-2; Asst. in Zoology, Hygienic Lab., U. S. Public Health & Marine Hosp. Serv., Sept. 1902 to June 1903; head of Zoological Lab., Bur. Animal Ind., 1903; became Chief of the newly created Zool. Div., Bur. Animal Ind., 1906; Ph.D., Univ. of Nebraska, 1908.

Dr. Ransom was U. S. Delegate to 7th International Zoological Congress, 4th Fisheries Congress, and 1st Pan American Scientific Congress; served on the editorial board of J. Parasit. and Am. J. Trop. Med.; member of 13 scientific societies; member Phi Beta Kappa and Sigma Xi; received Gold Medal of Seamen's and Tropical Diseases Research Assoc., Kobe, Japan, in recognition of his work on ascarids.

Dr. Ransom's work was otustanding and voluminous (a list of his publications includes more than 160 titles). He established basic facts in the life histories of many common parasites. An outstanding contribution to animal husbandry was his development of a swine sanitation system. As an executive he was a man of vision in his attitude toward his problems and was just, considerate, and generous in his dealings with his associates. He was uniformly courteous, kindly, and helpful, and possessed much personal charm.

Stiles, Charles Wardell. b. Spring Valley, N. Y., 15 May, 1867; d. 24 Jan., 1941. Student, Wesleyan College, Conn., 1885-6; Ph.D., Univ. Leipzig, 1890. Zoologist, Bur. Animal Ind., 1891-1902; U. S. Public Health and Marine Hospital Serv., 1902-10 as Zoologist and 1910-31 as Professor of Zoology. While working for the Bureau of Animal Industry, was Agriculture and Science Attache, U. S. Embassy, Berlin, 1898-99.

Dr. Stiles devoted much time to teaching and lecturing. He was Professor of Medical Zoology at Georgetown University, 1892-1906; Lecturer, Army Medical School, 1894-1901; and Lecturer, Navy Medical School, 1902-17. He gave occasional lectures at Johns Hopkins University Medical School from 1897 to some time in the 1930's.

Dr. Stiles was a man of many interests. He served as Secretary of the International Commission on Zoological Nomenclature from 1898 to 193-, was a corresponding member of the London Zoological Society, and a foreign

correspondent of the Societe Biologique and the Académie de Médicine—of France. In the field of Public Health, he inspired and motivated the hookworm eradication campaign in the southern United States, and in this connection was Scientific Secretary of the Rockefeller Sanitary Commission for eradication of hookworm disease, 1909-14. In 1919, he was commissioned Assistant Surgeon General (reserve), and took a great delight in wearing his uniform, complete with all embellishments, including a sword, whenever the opportunity arose. John L. Gardiner, Maybelle Chitwood, Marion M. Farr, and Doys A. Shorb.

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W. W. Cort	M. C. Hall*	C. W. Stiles*

*Deceased

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Attendance

Two hundred sixty members and friends of the Helminthological Society of Washington, including 31 graduate students and 10 guests, attended the scientific sessions, luncheon and banquet which were held at the University of Maryland, College Park, Maryland, on October 8, 1960, in commemoration of the 50th Anniversary of the founding of the Society on October 8, 1910.

Of the 213 registered scientists who attended the technical part of the meeting, 129 were from the District of Columbia, Maryland, and nearby Virginia, 14 from Michigan, 11 from Canada, 8 each from New York and Pennsylvania, 6 from Florida, 4 each from Massachusetts, North Carolina and West Virginia, 3 each from California, Illinois, and New Jersey, 2 each from Georgia, Ohio and Wisconsin, and 1 each from Delaware, Indiana, Kentucky, Maine, Missouri, New Hampshire, South Carolina, Puerto Rico, England, and Scotland. Included in this group from outside the United States were: Drs. Fairbairn, Harpur, Meerovitch, Myers and Tanner from McGill University, McDonald College, Quebec, Canada, Dr. Anderson from the University of Toronto, Ontario, Canada, Drs. Baker, Hopper and Mulvey from the Canadian Department of Agriculture, Ottawa, Ontario, Canada, Drs. Sommerville and Symons from the Commonwealth Scientific and Industrial Research Organization, Australia, Drs. Bowman and McIntyre from Aberdeen and the University of Glasgow, Scotland, respectively, Dr. Soulsby from the University of Cambridge, England, Dr. Lemma from Ethiopia, Drs. Gupta, Sen, Shirodkar and Nasir from India, Dr. Behin from Iran, Drs. Yamaguti, Oshima and Shiroishi from Japan, Dr. Vik from Norway, Dr. Bezubik from Poland, and Dr. Oliver-Gonzalez, from Puerto Rico.

Discovery of Trichinae and Determination of Their Life History and Pathogenicity*

BENJAMIN SCHWARTZ**

HOW TRICHINAE WERE DISCOVERED: The macroscopic lesions in human muscles produced by the nematode *Trichinella spiralis* were noted for several years before the causative organism was discovered. Some helminthologists believed that the whitish spots that Tiedemann saw in human muscles in 1822 were calcified cysts in which trichinae lodged. On the other hand, Leuckart and others did not regard calcifications that Tiedemann saw as having any relation to trichinae, because of their relatively large size and their location on the walls of blood vessels as well as in the muscles. Regardless of whether these calcifications were actually the cysts of trichinae, neither Tiedemann nor anyone else at that time even suspected that these spots were associated with or caused by a zooparasite. Neither is there any evidence that Peacock—who in 1828 made and deposited in the museum of Guy's Hospital in London a dry preparation of a human muscle beset with whitish specks, generally believed to have been the calcified cysts of trichinae—had any idea of the nature of the specimen he prepared.

The evidence at hand leaves no room for doubt that the credit belongs to Hilton (4), a prosector in Guy's Hospital, for first having suspected the parasitic nature of the small whitish spots that were occasionally seen in the muscles of cadavers during the fourth decade of the ninteenth century. In 1833, he not only discovered numerous whitish specks in a cadaver, but he actually investigated them and speculated on their nature. Although Hilton described the whitish specks as cysts, he was unable to discover any oragnization within them and regarded them as very small cysticerci. Whitish spots similar to those observed by him also were seen, as reported by Owen (11), during the ensuing two or three years by Wormald, demonstrator of anatomy in St. Bartholomew's Hospital, London. He regarded the spots as related to some "organized being." On January 30, 1835, Pagot (12) discovered numerous whitish cysts in the musculature of an Italian who had succumbed to tuberculosis in St. Bartholomew's Hospital. Wormald sent some of this material to Richard Owen who was keenly interested in all things parasitological. In the meantime, Paget, a first-year medical student, examined some of these spots, first with a hand lens, and later with a microscope. He determined that they were cysts and that nearly every one of them contained a small coiled worm. Apparently, there was no microscope in the hospital, so Paget, through the intervention of a friend in the British Museum, borrowed one from the botanist Robert Brown. It was Brown who dissected from one cyst a small worm, and Paget made a sketch of it. The discovery of whitish specks in 1835 by Paget, and a similar discovery by him a fortnight later in the body of a poor Irish woman who had died in the same hospital in a state of extreme emaciation, apparently as a result of a sloughing tumor, aroused so much interest in the anatomical laboratories of the hospital that Paget was invited to communicate his discovery to the Abernethian Society on February 6, 1835.

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*From the Animal Disease and Parasite Research Division, Agricultural Research Service, U. S. Department of Agriculture, Beltsville, Maryland.

This article was prepared to commemorate the hundreth anniversary of the discovery of the method of transmission and pathogenicity of *Trichinella spiralis*. It was read on January 20, 1960, at a meeting of Helminthological Society of Washington held in the Catholic University of America.

**Retired December 1, 1959. Present Address: 2480 16th Street, N.W., Washington 9, D. C.

Although the Transactions of the Society were not published, its detailed minutes were deposited in the hospital. Thirty-one years later, during a controversy over the discoverer of trichinae, two former presidents of the Abernethian Society (1) placed Paget's data on record in the Lancet. Briefly, Paget exhibited drawings of the parasite and noted that the minute, whitish specks were thickly scattered over the muscles. Also, he noted that the worms in small vesicles were threadshaped, blunt at both ends, and tapering towards an apparent head. He noted the outline of a contracted intestinal canal. Paget stated that there could be little doubt but that the organisms he found were identical with those described by Hilton in 1833; he noted, however, that they differed from Cysticercus.

Paget was only a freshman medical student when he discovered what afterwards was named *Trichina spiralis* by Owen and, by his own admission, he was not too familiar with zoology. Some of the muscles from the cadaver that Paget examined were sent to Owen, a well known zoologist who, according to Wormald (11), was "a keen hand for parasitical things from 'crabs' downward." Therefore, it is not surprising that it was Owen who named and described the worm (11). Actually, events moved so quickly that Owen's paper was read before the Zoological Society of London on February 24, only 18 days after Paget had presented his account of the same parasite before the Abernethian Society. Although Owen (10) gave an adequate description of the worm and named it *Trichina spiralis*, he confused the posterior end with the anterior one, he did not observe the intestine, and he believed that the parasite was not related to other known encysted nmatodes. He was unable to allocate *T. spiralis* to any of the then known groups of helminths. He regarded the worm as belonging to the Entozoa and as more or less related to the order of Cystica of Rudolphi. He expressed the opinion that *T. spiralis* belonged to another order in the lowest group of the animal kingdom and was allied to such genera as Spirillum and Vibrio.

The publication of Owen's paper soon lead to the discovery of trichinae in other cadavers in European countries. When Leidy (6) discovered in 1846 encysted worms in a piece of cooked ham he was eating, he recognized the parasites at once as trichinae when he viewed them under a microscope. He stated that he and others in the United States had previously seen trichinae in cadavers that had been necropsied. In the United States the first report of trichinae in man appears to have been made by Bowditch and Jackson (2) in 1842.

How the Mode of Transmission of Trichinae was Discovered: The source of trichinae and how they reached the human muscles remained unknown for 25 years following their discovery. It is true that the idea of spontaneous generation of insects and other small creatures had already fallen into considerable disrepute when trichinae were discovered, but a belief in *generatio aequivoca* of helminths still had adherents, even among students of worms. Owen stated that "an enfeebled state of the vital powers is the probable condition under which they (trichinae) are originally developed." He thought also that trichinae could increase in numbers within the muscle fibers by dividing. The theory that trichinae might be incompletely developed and, therefore, sexually immature worms, was first formulated independently by Dujardin and von Siebold in 1844, according to Leuckart (9). Up to that time, trichinae were considered adult worms, and the genital primordium was regarded as the gonad. von Siebold looked upon trichinae as juvenile erratic

worms of an adult unspecified human nematode. He believed that they had accidentally strayed into the muscles where they became trapped and were, therefore, destined to die and disintegrate there. Küchenmeister (5), who failed to infect dogs with trichinae, considered these worms to be the larvae of *Trichocephalus dispar,* now known as *Trichuris trichiura.*

In 1851 Herbst (3) found trichinae in the muscles of dogs to which he had fed the flesh of a badger infected with these parasites. Although there is no good reason for challenging the accuracy of Herbst's observations, the fact remains that he apparently confused other encysted nematodes, including those of cold-blooded vertebrates, with trichinae. Leuckart dismissed Herbst's experiments with trichinae having little, if any, merit.

An important step in attempts to unravel the life cycle of trichinae was taken by Leuckart in 1857 and published in detail later (7, 8). After failing to infect a dog by feeding it trichinous human flesh, he experimented with mice. One mouse, which died a day or so after swallowing such flesh, harbored in the intestine live trichinae that had escaped from the capsules. Another mouse, which died three days after a similar exposure, contained worms that had grown to twice their original size. Although Leuckart overlooked the significance of these feeding experiments and certainly gave little information concerning the morphology of the worms in the intestine of the mice, his observations showed that muscle trichinae of man could grow in the intestine of another mammal. Two years later, Virchow (13) carried out an experiment, published in detail in 1860 (14), that shed much more light on the developmental cycle of trichinae than any or all other experiments recorded up to that time. In July of 1859, Virchow fed human flesh containing encysted trichinae to a small dog. The dog, previously used in another experiment, died the fourth day after feeding, as a result of a condition not connected in any way with the trichina inoculum. At necropsy, Virchow found numerous macroscopic threadworms superficially on the intestinal mucosa but mostly free in the lumen of the lower duodenum and of the jejunum. The worms had the general appearance of trichinae, but they were much bigger and already contained sexual organs. The females contained eggs and the males contained spermatozoa. The eggs did not resemble the characteristically-shaped Trichuris eggs, and the posterior ends of the males were bluntly rounded. Spicules were not seen. Along with the mature worms he found several dead trichinae that also had grown beyond the size of the encapsulated worm and some worms that were still partially enclosed within their cysts. Although Virchow's data showed that encysted trichinae grew into mature worms that were different from Trichuris, he hesitated at first to differentiate the two species, partly because the developmental history of Trichuris was unknown at that time, and also because the adult trichinae he observed were only about three and a half days old. He did not overlook the possibility of a further morphological transformation that still might take place.

A month after Virchow's experiment had been reported to the Paris Academy in 1859, Van Beneden reported to the same Academy an account of experiments carried out by Leuckart (7) in 1858. Leuckart fed human trichinous flesh to a young pig and necropsied the animal four weeks later. A large number of Trichuris, some almost mature and others still incompletely developed, were found in the cecum and the colon. This finding apparently struck Leuckart as lending support to Küchenmeister's hypothesis that trichinae were incompletely developed Trichuris. After reading the account of

Virchow's experiment, Leuckart became convinced of the biological significance of the decapsulation and the subsequent growth of trichinae in the intestine of mammals, and he realized the importance of his earlier experiments with mice. Apparently, however, he was still unconvinced that the entire developmental cycle of the worm could take place in a single host and carried out the following experiment in 1859.

On necropsy of a dog to which he had fed human trichinous flesh a week earlier, he found in its intestine sexually-mature trichinae that had grown to three or more times their original size. Moreover, he saw that trichinae were viviparous since their uteri were filled with embryos. The manner in which he continued the experiment, however, showed that he still thought that a second host was necessary for the completion of the life history. Such a belief was not altogether inexcusable, because of the then newly-developed knowledge of the life cycles of taenioid tapeworms, to which Küchenmeister, in particular, and Leuckart, too, had contributed significantly. This knowledge lead Leuckart to assume that the trichina embryos that he had observed were destined to leave the host's intestine. On the outside, they were to be swallowed again by an intermediate host, just as the hexacanth embryos of Taenia, discharged from the bowels of the definitive host, develop into a bladderworm when swallowed by a suitable intermediate host. Leuckart fed the infected canine intestine to a young pig. He necropsied the pig five weeks later and found unencysted trichinae in the muscles. While this experiment was in progress he received a communication from von Zenker, along with a quantity of flesh from a young girl who had succumbed in the municipal hospital of Dresden, Germany, to what he determined to be a massive penetration of trichinae into her body musculature. The worms that Leuckart found in the pig were in the same stage of development as those he saw in the human muscles that von Zenker had sent to him. Leuckart also infected pigs by direct feeding of human trichinous flesh. He infected mice, cats, rabbits, and other animals and determined that trichinae were pathogenic to pigs and produced lameness in these animals. He became convinced also that *T. spiralis* was distinct from Trichuris and that the entire development of trichinae, from infective larva to infective larva, took place in one host. While Leuckart was engaged in experiments with pigs, Virchow (15) continued his investigations on the development of trichinae and used the human flesh that he, too, had received from von Zenker. In a feeding trial with a rabbit, he determined that trichinae were distinct from Trichuris and that their embryos escaped from the maternal uterus through the genital pore and finally wandered into the hosts' muscles and reached the primary muscle bundles. These observations were identical with those made by Leuckart. In short, Virchow demonstrated independently that the entire developmental cycle took place in one host and that a change of hosts, as earlier implied by Leuckart, was unnecessary. What still was left to be done in order to round out the developmental cycle of the worm in man, as Virchow saw it, was to discover adult trichinae in the human intestine. Even this gap was bridged very quickly by von Zenker. A few weeks after he had discovered the trichinae in the girl's muscles, he examined her preserved intestine and found sexually mature trichinae.

Thus it is seen that investigations carried out independently by Leuckart in Giessen, Virchow in Berlin, and von Zenker in Dresden lead to the same results. It is worth mentioning that on the same day that Virchow found trichinae in the muscles of the rabbit to which he had fed the flesh of the

girl, Leuckart (8) also found trichinae in the intestine of the pig to which he had fed, five weeks earlier, the gravid worms reared in a dog's intestine from encysted trichinae in human flesh.

How Trichinae were Shown to be Pathogenic: During the quarter of a century that elapsed between the discovery of trichinae by Paget and Owen and the recognition by von Zenker that these worms had a high pathogenic potential, they were looked upon by the medical man, who found them while performing necropsies, merely as zoological curiosities. The only exception was Wood (16) who, less than two months after Owen read his paper on *T. spiralis,* sent a communication to the Lancet in which he reported the death of a young man one year earlier in whose muscles trichinae were discovered during a necropsy. The subject, a robust and vigorous-looking man of 22, died in the Bristol Infirmary in 1834, about a week after admission. Wood observed numerous trichinae in the deltoid and pectoral muscles and stated that the parasites were similar to those described by Owen. He noted that the subject was admitted to the Infirmary with an attack of acute rheumatism, so violent in nature that he was unable to support himself and had to be brought there on the back of his father. About a fortnight before admission he began to show some indisposition, following which he developed increasing pain in his limbs. He was troubled with a cough and dyspnoea and kept to his bed for six days before admission. In the hospital venesection was done six times, involving the withdrawal of 16 ounces of blood at each bleeding. Post-mortem examination showed pneumonia, pericardial inflammation, and the muscle involvement due to trichinae. The patient was sick three weeks before death. Even assuming that he acquired the infection a week or so before he became ill, it is difficult to see how Wood could have discovered trichinae in the muscles because he made no reference to seeing whitish spots or specks. Since Wood stated than he conducted "an ordinary post-mortem examination" it must be assumed that what he saw was macroscopically visible—either cysts that already were calcified, in which case the young man did not suffer from an acute attack of trichinosis, or else other lestions, perhaps caused by the recent penetration of trichinae into the muscles. At any rate, Wood's suggestion that the rheumatic condition from which this individual suffered might have been related to the trichinae he harbored appeared to have made no impression on anybody until 1860, when his observations were brought to light again.

In January 1860, von Zenker (17) reported the death of a young servant girl who had been ailing since the previous Christmas eve. The girl was admitted to the municipal hospital of Dresden with severe muscle pain nearly three weeks after the onset of illness. She died two weeks after admission. In addition to experiencing fatigue, insomnia, loss of appetite, constipation, fever and thirst, she had severe pain in the muscles. Despite the fact that there was no swelling of the spleen, and no rose spots, the illness was diagnosed as typhoid fever, because the entire clinical picture appeared to be more nearly related to typhoid fever than to any other disease known at that time. The infection appeared to involve the whole muscular system, and the patient suffered such extreme pain, especially at the extremities, that she yelled day and night. Her knees and elbows were in such a state of contraction that each attempt to stretch them was unsuccessful because of the sharp pain it caused. Shortly before death there were pneumonic symptoms, which suggested a lung infection, and this was followed by a striking apathy that terminated in death 24 hours later.

At necropsy, one day after death, von Zenker noticed that the muscles were of a pale red-gray color and partly spotted. Microscopic examination showed dozens of unencapsuled trichinae, some stretched out and showing definite signs of life. Further investigation showed all muscles in a similar condition. With a low magnification of the microscope, he found up to 20 worms in one field. He concluded that he had caught the organisms in the act of penetrating into the muscles. The muscle bundles showed throughout a high degree of degeneration, including the disappearance of the cross striations of the fibers. There was no swelling of the spleen, no involvement of the intestinal and mesenteric lymph glands, and no other pathological changes suggestive of typhoid fever. Later, he noted a collapse of the left lung with some infiltration, an intensive bronchitis, and a strong hyperaemia of the mucosa of the ileum. von Zenker concluded that the penetration of the trichinae into the muscles was the cause of the intense muscle symptoms. The trichinae that he examined had the same morphology as that he previously had seen in the worms from muscles of cadavers. Several weeks later, von Zenker laid out the girl's intestine, which he had kept in a state of preservation. To his "joyful surprise" he found in the first few drops of mucus from the jejunum a mass of worms from 1.5 to 4 mm. long with a morphology that left no doubt that they were sexually-mature trichinae. He noted, as Leuckart and Virchow had done previously, that the females were viviparous. von Zenker realized that his observations on a naturally-occurring case of human trichinosis were in agreement with the experimental results obtained by Leuckart and Virchow. As a matter of fact, considering the delay between von Zenker's necropsy of the girl's body and the later examination of her intestine, it is not impossible that the thought of examining the intestine for adult trichinae was not an original idea that came to him. It is greatly to his credit, however, that he was not completely satisfied to rest his laurels on the pathological and zoological observations he had made, but decided to pursue the matter further in the hope of ascertaining, if possible, the source of the infection. It is important to bear in mind at this point that Leidy first discovered trichinae in pork in the United States 14 years earlier. There is reason to believe that he assumed that man would acquire these parasites by eating pork harboring trichinae. It is clear also that Leidy's discovery was known in Europe. At any rate, because the girl had been a servant on the farm where she became ill right after Christmas Eve, von Zenker stated that he thought she must have eaten pork about that time, in accordance with the custom of country folks. He therefore went to the country to interview the girl's former employer, whom he found to be very intelligent and cooperative. The employer admitted that on the 21st of December he had a butcher slaughter a pig and a beef animal. Shortly after this, the girl became sick. He could not state with certainty whether she ate the raw meat but stated that she was a "nibbler" and was inclined to taste and eat food that she handled. von Zenker was able to secure pieces of ham and sausage from the pig, the meat of which the victim had evidentally eaten. The first preparations he made of the ham and sausage showed numerous encysted trichinae. Ham from another pig killed in November, and meat from the bovine killed in December were free of trichinae. Later, von Zenker pushed his investigation further in order to determine whether others on the farm also became ill. He discovered that the girl's employer and wife were sick for a short time in January, and had symptoms suggestive of intestinal and muscle involvement. He even visited the home of the butcher who had slaughtered the animals on the farm, and dis-

covered that he, too, had been very sick in January, was confined to his bed for at least three weeks, and that his whole body became lame. As a matter of fact, the butcher made a slow recovery from his illness and appeared to lack the strength that he formerly had. His illness, too, developed shortly after he had slaughtered the pig. von Zenker remarked that it was the habit of butchers in Germany to taste the raw meat of the animals they slaughtered. It appears, therefore, that von Zenker's investigation was thorough and that his epidemiological studies brought to light facts heretofore unknown—facts that pointed to the great danger of eating uncooked pork, a custom to which the German people were definitely addicted.

There can be no doubt that the credit for demonstrating the pathogenicity of *T. spiralis* belongs to von Zenker. Not only were his laboratory investigations carried out patiently and completely, but by his studies outside of the laboratory he unravelled the source of infection. The credit for unravelling the complicated life history of the parasite is shared by Leuckart and Virchow. The latter apears to have made a rather direct approach to the solution of a unique life history. However, once Leuckart had freed himself from the spell of Küchenmeister's theory that trichinae were the offspring of Trichuris, he, independently of Virchow, followed the clear path that led to the understanding of how trichinae were transmitted, how they developed and reproduced, and how their brood invaded the muscles of the same host in which they were born.

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A new Echinostome, *Parallelotestis kafuensis*, n. sp., from the Great White Heron, *Egretta alba melanorhyncha* (Wagl.), in Northern Rhodesia.

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A Great White Heron, *Egretta alba melanorhyncha* (Wagl.), was shot on the River Kafue above Kafue Gorge, Northern Rhodesia in September 1959. On examination, the gall bladder was found to be packed with echinostomes. The majority of these (36) was identified as *Pegosomum spiniferum* Ratz, 1903 while the remaining 13 appear to represent a new species of the genus *Parallelotestis* Belopolskaia, 1954 for which the name *kafuensis* is proposed.

The trematodes were fixed in cold formal-acetic under slight coverslip pressure and stained with Kirkpatrick's carmalum or Erhlich's haematoxylin. Unless otherwise stated all measurements are in millimeters.

Description: Body 5.57-8.08 long by 2.08-2.96 maximum diameter in testicular region (Fig. 1). Posterior end of body attenuated. Cuticle spinous over whole body except in region of genital pores; cuticular spines up to 98 microns long by 38 microns wide. Head collar 0.65-0.87 diameter with 27 head spines. Corner spines: 2 ventral groups with 4 spines in each, 2 oral and 2 aboral; lateral aboral spines, 140-192 microns long by 42-63 microns wide, largest of whole series; other corner spines 112-158 microns long by 46-62 microns wide (Fig. 2). Lateral marginal spines: 2 groups with 6 spines in each arranged in a single row; 88-115 microns long by 28-38 microns wide. Dorsal spines: 7 in number, arranged in a double row, 4 oral and 3 aboral; median spine aboral (Fig. 3). Oral spines of dorsal series 67-100 microns long by 25-35 microns wide; aboral spines 91-115 microns long by 32-42 microns wide. Oral sucker terminal, 0.15-0.19 long by 0.17-0.25 diameter; prepharynx up to 0.12 long; pharynx 0.26-0.31 long by 0.19-0.25 wide; oesophagus variable in length, up to 2.73 long, with irregular lateral diverticula; intestinal caeca extend almost to posterior end of body. Ventral sucker, 0.75-1.03 long by 0.73-0.98 diameter, lies in anterior half of body. Testes irregular or transversely oval in shape, lying oblique to each behind ovary; never absolutely symmetrical. Left testis, 0.27-0.48 long by 0.32-0.56 wide, always slightly anterior to right testis, 0.32-0.50 long by 0.34-0.51 wide. Cirrus sac, 0.63-0.87 long by 0.55-0.64 wide, almost completely preacetabular. Cirrus armed and powerful, up to 0.79 long by 0.46 wide (Fig. 4). Cirrus spines up to 18 microns in length by 11 microns wide. Ovary rounded, 0.24-0.42 diameter, situated to right of mid-line. Mehlis' gland diffuse to left of ovary. Uterus forms 2 conspicuous transverse slings posterior to ventral sucker and a third dorsal to ventral sucker. Metraterm opens to left of cirrus by a separate female pore. Genital pores situated between ventral sucker and intestinal bifurcation. Vitelline follicles extend from a level anterior to ventral sucker almost to posterior end of body; follicles tend to fill post-testicular region. Transverse vitelline ducts join medianly to form small vitelline

reservoir. Excretory pore terminal. Eggs 88-105 microns long by 53-67 microns wide.

HOST: *Egretta alba melanorhynchus* (Wagl.).
LOCATION: Gall bladder.
LOCALITY: Kafue River, near Kafue, Northern Rhodesia.
CO-TYPES: To be deposited in the British Museum (Natural History).

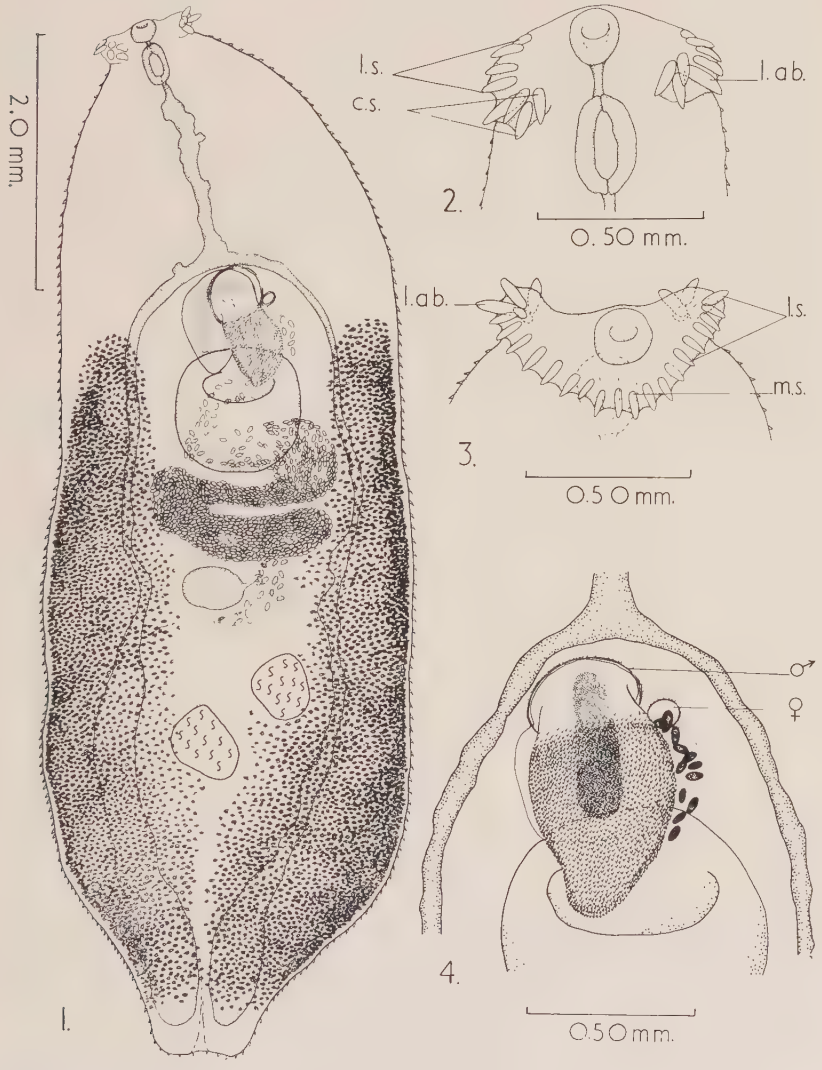

Fig. 1. *Parallelotestis kafuensis*, n. sp. Entire specimen, ventral view.
Fig. 2. Ventral view of head collar showing corner spines.
Fig. 3. Dorsal view of head collar showing lateral and dorsal spines.
Fig. 4. Cirrus sac, extruded cirrus, male and female pores.
Legend: c.s.—corner spines; l.ab.—lateral aboral spine; l.s.—lateral spines; m.s.—median spine of dorsal series.

Discussion: According to Skrjabin (1956) the genus *Parallelotestis* was erected by Belopolskaia in 1954 to accommodate *P. horridus* which was recovered from the gall bladder of *Egretta alba modesta* in Russia. The diagnostic criteria used to separate *Parallelotestis* from other genera of the subfamily Echinostomatinae include the symmetrical position of the testes and the preacetabular position of the cirrus sac. In the present material the testes are not absolutely symmetrical, but are placed obliquely in the posterior half of the body. It is suggested that the generic diagnosis quoted by Skrjabin (1956) should be revised to include forms in which the testes are either symmetrical or oblique.

Yamaguti (1958) gave a key separating the genera of the subfamily Echinostomatinae that occur in birds. In this key *Parallelotestis* is separated from *Echinoparyphium* Dietz, 1909, on the difference in the position of the testes and on the supposition that the genital pore is "prebifurcal" in the former genus. Although Skrjabin (1956) using Belopolskaia's original description of *P. horridus* does not mention the position of the genital pore it would appear from the figure and the measurements given that the genital pore is posterior to the intestinal bifurcation, as in the present material, and not anterior as stated by Yamaguti.

Srivastava (1958) proposed a new genus, *Proechinocephalus,* designating *P. tarai* as genotype. *P. tarai,* was described from a single specimen with 23 head spines recovered from the intestine of an Indian Egret, *Bulbulcus ibis coromandus.* Apart from the difference in the number of head spines *P. tarai* is very similar to *Parallelotestis horridus* and it is suggested therefore that the genus *Proechinocephalus* Srivastava, 1958 is a synonym of *Parallelotestis* Belopolskaia, 1954. Srivastava (1958) considered that *proechinocephalus* was "most closely related to *Chaunocephalus* Dietz, 1909 on account of the symmetrical disposition of the testes in which character it stands apart from all the other genera of the family." Srivastava seems to have overlooked the fact that the subfamily Chaunocephalinae Travassos, 1922 is separated from other echinostomes on the division of the body into a bulbous anterior region and a cylindrical posterior region.

In possessing larger head and cuticular spines *P. kafuensis* differs from *P. horridus* and *P. tarai.* The cirrus sac and cirrus are also larger in the present species than in the previously described forms.

Summary

A new species of echinostome, *Parallelotestis kafuensis,* n. sp., from *Egretta alba melanorhynchus,* (Wagl.), is described.

The proposed synonymy of the genus *Proechinocephalus* Srivastava, 1958 with *Parallelotestis* Belopolskaia, 1954 is discussed.

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Studiees in Cysticercoid Histology. III Observations on the Fully Developed Cysticercoid of *Raillietina cesticillus* (Cestoda: Cyclophyllidea)

MARIETTA VOGE

The cysticercoid of *Raillietina cesticillus* (Molin) has been studied by several workers. Wisseman (1945) described the development of this tapeworm in ground beetles. Wetzel (1934) gives an account of the histology of developmental stages and of the fully developed cysticercoid. Because my observations are not in complete agreement with those of Wetzel, studies on the histologic structure of *R. cesticillus* are presented here.

MATERIALS AND METHODS

Raillietina cesticillus was collected from chickens in Honolulu, Hawaii. Gravid proglottids were fed to *Tribolium confusum* raised in enriched flour, and the beetles kept at room temperature for one month. Beetles were dissected in saline solution and the cysticercoids fixed in Zenker's fluid or in 10% formalin. Observations on living material were made using the light microscope. Fixed cysticercoids were embedded in paraffin and sections cut at 7 microns. Stains used were Mallory's anilin blue collagen stain (Gridley, 1953), Mayer's hemalum, van Gieson's collagen stain, and alizarin red and von Kossa's stain for the demonstration of calcium (Lillie, 1954). In addition, Himes' triple stain (Himes and Moriber, 1956) was used to great advantage because it incorporates specific procedures for protein, DNA and polysaccharide. With this stain, extra-nuclear protein appears yellow, chromosomes green or blue, and polysaccharide red. For the latter, control sections were treated with malt diastase as directed by Lillie (1954). Sections were also examined under polarized light to test for birefringence.

OBSERVATIONS

The histologic structure of the cysticercoid of *Raillietina cesticillus* is shown in figure 1 which represents a longitudinal section of this organism. The ruffled appearance of the surface is also characteristic of living material. The cysticercoid is surrounded by a thin, transparent coat which stains a very pale blue with Mallory's stain and cannot be observed in sections stained with Himes' triple stain. This hyaline coat is perhaps responsible for the "slippery" quality of living cysticercoids. Beneath the coat is the external membrane, a relatively firm and deeply staining sheet in which are embedded numerous small granules. These stain deep red with Mallory's triple, bluish-grey with Mayer's hemalum, and are not visible in sections stained with Himes' triple stain. The external membrane stains a deep purple with Himes' stain, suggesting the presence of polysaccharide. Beneath the membrane is a layer of transversely oriented circular fibers arranged in bundles, as described for the cysticercoid of *Hymenolepis nana* (Voge and Heyneman, 1960). Among the fibers of this layer are numerous small granules more or less the size and shape of those in the external membrane. The composition of these bodies could not be determined. They do not react with von Kossa's stain or with

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Appreciation is expressed to Dr. Joseph Alicata, University of Hawaii, Honolulu, for his generous help in the recovery of the strain of *Raillietina;* and to Mrs. Nora Liu for technical assistance.

alizarin red which suggests that they do not contain calcium. They stain bright red with Mallory's stain and greyish-blue with Mayer's hemalum. Himes' stain shows small, yellow areas of about the same size and shape as the granules, but less sharply defined. Granules are also seen among the fibers of the outer longitudinal fibrous layer (Fig. 1). Neither the circular fibers nor the outer longitudinal fibrous layer contain any nuclei. All fibers stain an intense reddish-purple with Himes' stain and a brilliant blue with Mallory's stain. Beneath the outer longitudinal fibrous layer is the intermediate layer which contains many nuclei with or without surrounding cytoplasm, as well as the small granules described above. In addition, there are a few large granules which show a brownish precipitate with von Kossa's stain and a yellow color with Himes' stain. It is therefore likely that these large bodies represent "calcareous corpuscles," perhaps composed of a protein core around which calcium, probably calcium phosphate, has been deposited. Among the nuclei and granules of the intermediate layer, there are bundles of fibers, the fibrous processes, which connect the outer longitudinal fibrous layer with the inner longitudinal fibrous layer. The latter is situated beneath the intermediate layer (Fig. 1) and consists of densely packed fibers, as well as nuclei. While most of these fibers are oriented longitudinally, some of them appear to be oriented at random. A relatively heavy outgrowth of fibers from this layer is seen in the posterior end of the cysticercoid, where the separations between layers are indistinct or completely obliterated. The inner longitudinal layer borders the cysticercoid cavity. In living cysticercoids, the cavity is packed with relatively large bodies which completely obscure the scolex. These bodies dissolve within one minute upon the addition of 5% HCl and disappear in material fixed with Zenker's. Formalin fixation preserves them, and treatment with von Kossa's silver stain produces a brown to black precipitate. It is therefore likely that these granules contain calcium.

The tissues within the cysticercoid cavity are the lining of the cavity (Fig. 1), a delicate mesh of fine strands containing nuclei, and the scolex with surrounding tissue. While all fibers external to the cavity appear brilliant blue with Mallory's, the fine strands inside the cavity, as well as in the scolex, stain purplish-pink or dull greyish-blue. With Himes' stain, extra-nuclear tissue stains purple and the nuclei the usual green or blue. The cuticle of the scolex, which in *Hymenolepis diminuta* stains a deep red with Mallory's stain (Voge, 1960), is grey-blue in *Raillietina cesticillus*. The hooks of the rostellum stain a bright orange, but appear colorless with all other stains. When sections are examined under polarized light, the rostellar hooks are the only structures which are birefringent.

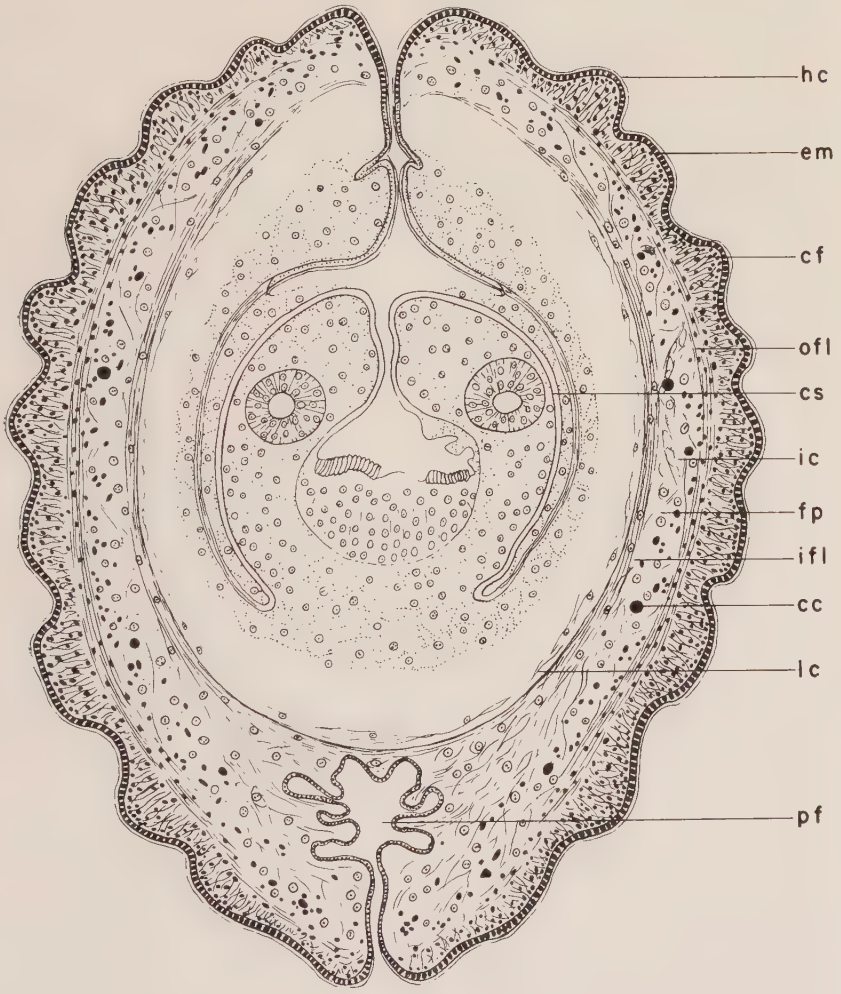
Discussion

The present description of the cysticercoid of *Raillietina cesticillus* differs from that of Wetzel (1934) in several respects, the major difference being the interpretation of structures. Wetzel states that the cysticercoid is surrounded by a thin, cuticula-like envelope, probably furnished by the host. However, there is no evidence that any of the tissues figured by Wetzel or shown here in figure 1 represents a contribution from the insect host. Adherence of cysticercoids to insect tissues was never observed, and the over-all structure of the cysticercoid is similar to that observed in other species in which tissue contribution from the host has not been demonstrated.

At the posterior end of the cysticercoid there is a relatively large cavity

which opens to the outside. Wetzel refers to this space as the excretory bladder. Since there is no evidence that this structure is connected with excretory functions, the term "posterior fold" is used here.

Of interest is the large number of small granules in the tissues outside the cysticercoid cavity. Do they perhaps represent degenerative stages of whole cells or of nuclei which had contributed to the formation of the fibrous layers? If this were so, one could expect to find similar structures in the fibrous layers of other cysticercoids. However, in *Hymenolepis diminuta* and *H. nana* granules of this type are not present. A careful histogenetic study might help



Free-hand drawing of longitudinal section of *Raillietina cesticillus* cysticercoid, showing arrangement of different tissues: cc, calcareous corpuscle; cf, circular fibers; cs, cuticle of scolex; em, external membrane; fp, fibrous processes; hc, hyaline coat; ic, intermediate cell layer; ifl, inner fibrous layer; lc, lining of cavity; ofl, outer fibrous layer; pf, posterior fold.

to clarify this problem. In the past, many small bodies in tapeworm tissues have been called calcareous corpuscles on the basis of insufficient evidence.

Regarding the demonstration of polysaccharide with Himes' stain, results obtained with malt diastase digestion show that the scolex and surrounding tissues contain polysaccharide. The external membrance, however, contains polysaccharide not affected by this enzyme, or perhaps another substance giving a similar staining reaction. The fibrous tissues all appear brown or nearly colorless after malt diastase digestion. Further histochemical studies are needed for a complete analysis of polysaccharides in *Raillietina* cysticercoids.

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Sasser, J. N. and W. R. Jenkins. Nematology: **Fundamentals and Recent Advances with Emphasis on Plant Parasitic and Soil Forms.** This volume, edited by Sasser and Jenkins, was compiled from material presented during the summer of 1959 during the Southern Regional Graduate Summer Session in Nematology held at North Carolina State College. Twenty-two scientists contributed the several sections. Except for more general coverage of morphology, anatomy, physiology, and biochemistry of nematodes, emphasis is on the biology and control of plant parasitic nematodes. University of North Carolina Press, Chapel Hill, North Carolina, 1960, 480 + XV pp. $12.50.

A new cestode, *Ophiotaenia ophiodex*, n. sp., from a Night-Adder, *Causus rhombeatus* (Licht.), in Southern Rhodesia

David F. Mettrick
Department of Zoology, University College of Rhodesia and Nyasaland.

In September, 1958, a Night Adder, *Causus rhombeatus* (Licht.), was brought into the department from the College Farm, near Salisbury. On examination a number of cestodes were found in the hind-gut. These appear to represent a new species for which the name *Ophiotaenia ophiodex* n. sp. is proposed. Subsequently some further specimens were recovered from the intestine of a White-bellied Grass-Snake, *Psammophylox tritaeniatus* (Gunther).

Ophiotaenia ophiodex, n. sp.

All measurements in mm. unless otherwise stated.

Description: Medium sized worms; length 21-27 cms., width 0.88-0.97. Scolex 0.79-1.14 in diameter; rostellum absent; scolex completely unarmed. Four suckers, 0.38-0.46 in diameter. Mature segements much longer than wide. Typical mature segment 0.81 wide and 3.09 long. Genital pores lateral; alternate irregularly; open slightly posterior to the equatorial level of the segment. Typical gravid segment 0.91 wide and 3.41 long. Testes numerous, 110-120 per segment in two lateral bands extending entire length of segment. Number of testes in aporal band 58-63; poral band 52-57. Diameter of testes 48-56 microns. Cirrus-sac pear shaped, 0.19-0.20 long by 0.12-0.13 wide; opens into genital atrium either anterior or posterior to the vagina. Ratio of length of cirrus-sac: segment width 1: 4-4.5. Ovary bipartite, in posterior region of the segment, maximum width 0.63. Two lobes joined by a narrow isthmus. Lateral margins of lobes approach lateral margins of segment. Vagina 17-19 microns in diameter, straight, thick walled. Vitelline glands in two lateral bands, extending entire length of segment. Number of follicles in aporal band 117-121; poral band 88-101. Diameter of follicles 18-24 microns. Transverse vitelline duct just posterior to ovary. Oviduct arises posteriorly, and dorsally. Receives vagina and yolk duct. Mehlis' gland 0.59-0.62 in diameter, lies posterior to ovary. Uterus coiled in region of Mehlis' gland just before passing anteriorwards; median in position; runs forward almost to anterior margin of segment. In gravid segments digitiform lateral pouches appear, 30-42 on either side of median stem of uterus. Eggs 36 microns long by 27 microns wide. Onchospheres 18-20 microns in diameter.

Hosts: Night Adder, *Causus rhombeatus* (Licht.), and the White-bellied Grass Snake, *Psammophylox tritaeniatus* (Gunther).

Location: Hind intestine.

Locality: Near Salisbury, Southern Rhodesia.

Type to be deposited in the British Museum (Natural History)

Discussion: La Rue (1914), in his monograph on the Proteocephalidae, erected the genus *Ophiotaenia*, which since then has had a somewhat checkered career. It is proposed to accept *Ophiotaenia* as a useful generic grouping, while recognising that in a number of *Ophiotaenia* species, particularly those which are parasites of Amphibia, it is difficult to decide whether they should correctly be placed in the genera *Ophiotaenia* or *Proteocephalus*.

Both these genera have been divided into species groups, *Proteocephalus* by Meggitt (1927), and *Ophiotaenia* by Wardle & McLeod (1952).

The present material falls into Wardle & McLeod's Species Group VI. In this group, which has the following characters "testes fewer than 200, cirrus-sac one third or less of the segment width, and more than 25 uterine branches each side," there are 19 described species.

Only four species of the group have been recorded from Africa: *O. gabonica* (Beddard, 1913), *O. adiposa* and *O. theileri* (Rudin, 1917), and *O. elapsoideae* Sandground, 1928. Beddard does not give either a figure or any measurements of systematic importance for *O. gabonica,* which species should therefore be transferred to the "incertae sedis" group, pending a redescription of the type. Sandground also gives a re-description of *O. gabonica.* He admits that Beddard's description is very incomplete, but assigns some fragmentary material to *O. gabonica* because it came from the type host, and the measurements were very similar. If this re-description is accepted then *O. gabonica* has 137-170 testes, the cirrus-sac is 0.57 mm. long by 0.23 mm. wide, and there are 38-46 lateral uterine branches.

O. adiposa has 170-210 testes, 40-50 uterine branches, and the ratio of cirrus-sac length : segment width is 1 :4.5. *O. theileri* has 160-310 testes, 25-40 uterine branches, and a similar cirrus-sac length : segment width ratio. *O. elapsoideae* has 100-125 testes, 48-55 uterine branches, the ratio of cirrus-sac length : segment width is 1 :3-3 :5, and the cirrus-sac is 0.37mm. long by 0.16mm. wide.

Ophiotaenia ophiodex, n. sp. may be separated from these four species, and also the other fifteen in the group on the basis of number of testes, size of the cirrus-sac, and its relationship to the total width of the segment, position of the genital pore, and the number of uterine branches present.

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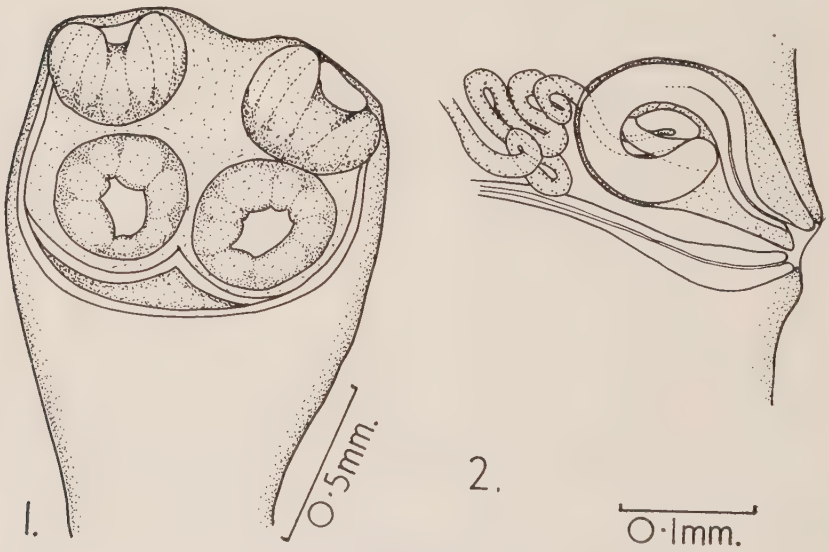

Ophiotaenia ophiodex, n. sp.
Fig. 1. Scolex; Fig. 2. Cirrus-sac and Metraterm.

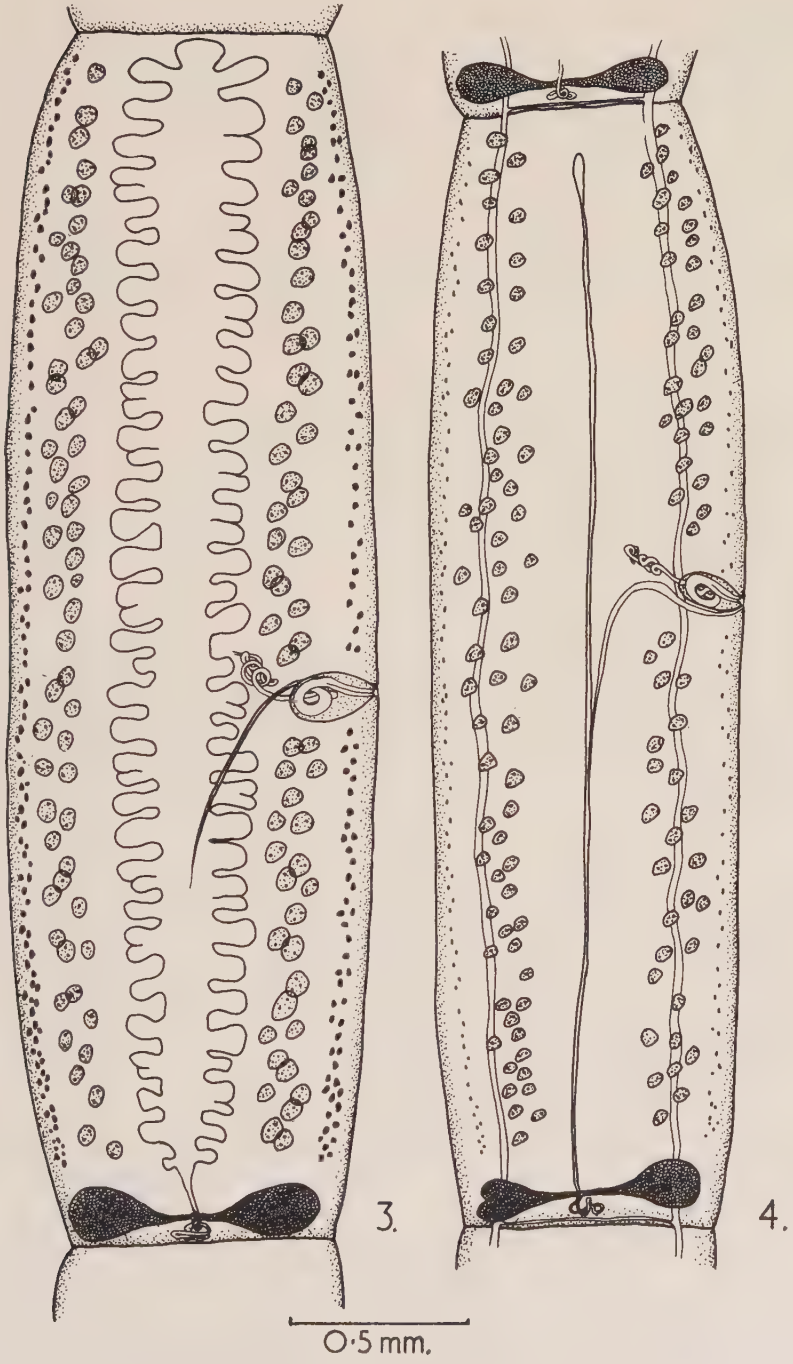

Fig. 3. Gravid segment; Fig. 4. Mature segment.

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Alliea laruei, n. gen., n. sp., (Acarina: Cheyletidae) from *Rattus norvegicus* (Erxleben) in Florida*

Conrad E. Yunker**

The family Cheyletidae contains some species that are obligate parasites, some that are facultatively parasitic, and some that are voracious, free-living predators of other small arthropods. The great majority of species in the family, however, have not been studied from a biological aspect, and nothing is known of their feeding habits. A number of the latter are commonly found on rabbits, rats, squirrels and birds, and in their nests. The specimens described herein were taken from two *Rattus norvegicus* (Erxleben) in Florida.

Alliea, n. gen.

Cheyletidae; oval in shape, with two large dorsal shields, each with more than 12 pairs of squamiform setae; without eyes; base of rostrum covered by a tectum originataing on gnathosomal base-ring; palpal genu insensibly fused dorsally with femur; palpal claw without teeth; palpal tarsus without comblike setae: with one large and one small sicklelike seta, an inflated seta and a small spathulate seta; sensory rod of tarsus I located in terminal third of segment.

Type-species: *Alliea laruei*, n. sp.

Remarks: The new genus superficially resembles *Eucheyletia* Baker, 1949, but members of the latter genus possess comblike setae on the palpal tarsus of both males and females, and cloudlike mid-dorsal setae on the idiosomal shields; in addition, the palpal genu of *Eucheyletia* spp. is strongly demarcated from the femur.

The new genus is named in honor of Mrs. Allie M. Brown, Department of Zoology, University of Maryland, who has contributed greatly to the acarological efforts of that Department.

Alliea laruei, n. gen., n. sp.

Holotype male.—An oval mite with elongate palpi. Length, measured dorsally exclusive of gnathosoma, 246 microns; width, measured dorsally at midpoint, 190 microns.

Idiosoma: Dorsum almost completely covered by two semicircular shields, a propodosomal and a hysterosomal that meet and are contiguous at midpoint; surface of shields punctate-striate. Propodosomal shield with 12 pairs of

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Figure 1. *Alliea laruei*, n. sp.: (a) gnathosoma, male; (b) palpal tibia and tarsus, male, enlarged ventral view; (c) dorsum, female; (d) male, dorsal view; (e) tarsus I, female; (f) venter, male.

squamiform, serrate setae; hysterosomal shield with nine pairs of similar setae and three pairs of smaller, narrower, serrate terminal setae. Eyes absent. Integument striate, with a single pair of squamiform, serrate lateral setae. Venter striate, with five pairs of short simple setae and two pairs of similar anal setae. Anus a short subterminal slit.

GNATHOSOMA: Elongate; length, measured dorsally from posteromedian border to tip of rostrum, 90 microns; width, measured dorsally at anterolateral margins of fused coxae-trochanters, 104 microns. Gnathosomal base-ring indistinctly rugose; elongated anteriorly to form a tectum over base of rostrum. Peritreme not segmented; shaped like an inverted "U." Rostrum pyramidal, with one lateral and one ventral pair of simple penultimate setae. Femur elongate, more than two times longer than wide; surface rugose; bearing an elongate, squamiform, serrate dorsal seta arising from a base set in a tuberculate projection of femur, and two elongate, simple ventral setae. Genu coalesced and merging insensibly with femur on dorsal and outer aspect; with an elongate, squamiform, serrate dorsal seta and a shorter, simple ventral seta. Tibia with a claw and four setae; two elongate, simple dorsal setae, an elongate, ventral seta, and a short, blunt, club-shaped sensory rod at base of tarsus. Palpal claw elongate, narrow and without teeth. Tarsus short, narrow; arising from inner and ventral aspect of tibia; with four setae: an elongate, heavy, bare sicklelike seta, a shorter bare sicklelike seta, a short, broad, inflated seta shaped like a candle-flame, and a short spathulate seta. LEGS: Moderately long and radially arranged; coxae I and II separated from III and IV by a short space; all segments striate.

(I) Coxa overlaid with ventral integumental striations; posterior margin contiguous with coxa II; with two short simple setae. Trochanter short, wide; bearing a single apical seta. Femur long, wide, convex dorsally; with a squamiform, serrate dorsal seta similar to those of dorsum, and an elongate, simple ventral seta. Genu rectangular in outline, shorter and narrower than femur; with two squamiform, serrate, dorsal setae and a single, minute, blunt, dorsal sensory rod. Tibia similar to genu; bearing six setae: one small and two large squamiform, serrate dorsal setae, a small, blunt, dorsal sensory rod somewhat longer than that on genu, and two elongate, simple ventral setae. Tarsus comprising one-third the length of leg; broad and striate proximally; becoming narrower and punctate-striate distally; terminating in a setiferous, knoblike dorsal eminence and a claw-bearing stalk; with 10 setae: an elongate, squamiform, serrate dorsal seta arising in basal third of segment, an elongate, simple ventral seta arising in medial third of segment, a moderately long blunt, dorsal sensory rod arising in terminal third of segment, one short, one slightly longer, and two elongate setae arising from setiferous, knoblike dorsal eminence, one moderately long, very fine seta on ventrolateral aspect of terminal third of tarsus, and two ventral setae on terminal third of segment, one of these short and simple, the other longer and bearing fine setules. Posttarsus consisting of paired claws and a padlike empodium having a double row of 4-6 tenent hairs.

(II) All segments similar in shape to those of leg I, but proportionately smaller in size; coxa contiguous with coxa I; overlaid with ventral integumental striations. Trochanter and femur as in leg I. Genu with a squamiform, serrate dorsal seta, a minute, blunt, dorsal sensory rod and a simple dorsal seta that bears fine setules. Tibia with two squamiform, serrate dorsal setae, a short, blunt, dorsal sensory rod, and two simple ventral setae. Tarsus

with eight setae: a moderately long, blunt, striate sensory rod arising in mid-dorsal region, a seta with setules arising from corresponding ventral region, two elongate setae arising from setiferous knob, a shorter simple lateral pair and a similar ventral pair, the latter bearing setules, arising from base of claw-bearing stalk. Posttarsus similar to that of leg I.

(III) Coxa separated from coxa II by a space equal to width of coxa; contiguous with coxa IV; overlaid with ventral integumental striations; with two short, simple setae. Trochanter longer than wide; with a squamiform, serrate dorsal seta and a simple ventral seta. Femur longer than wide, convex dorsally, with two squamiform, serrate dorsal setae, one noticeably smaller than the other. Genu rectangular in outline, with a squamiform, serrate dorsal seta and a similar ventrolateral seta. Tibia similar to genu, proportionately smaller; with two squamiform, serrate dorsal setae and two elongate, simple ventral setae. Tarsus with eight setae: a comparatively elongate, striate, blunt sensory rod arising from basal third of ventral surface of segment, an elongate simple seta proximal to latter on ventral surface, two elongate, simple setae arising from dorsal setiferous knoblike eminence, a simple lateral pair and a pilose ventral pair of setae arising from base of claw-bearing stalk. Posttarsus similar to that of leg II.

(IV) Similar to leg III with following exceptions: trochanter with a single, simple ventral seta; genu with two squamiform, serrate dorsal setae, one large and one small.

Female: Similar to male with following exceptions. Length, measured dorsally exclusive of gnathosoma, 305 microns; width, measured dorsally at midpoint, 207 microns. Propodosomal shield with 16 pairs of squamiform, serrate setae; hysterosomal shield with 15 similar pairs. Genital opening a longitudinal slit, bordered on either side by a striated integumental flap, each of these having three short simple setae; bordered posteriorly by a punctate-striate semicircular area bearing three pairs of setae similar to those on either side of genital opening. Gnathosoma not seen.

Legs: Genu I with a single, squamiform, serrate dorsal seta and a lanceolate pilose seta (presence or absence of sensory rod indeterminable). Tarsi III and IV without sensory rod. Posttarsi III and IV each with 8-10 rows of tenent hairs arising from padlike empodium.

Type Material: Holotype male and allotype female deposited in U. S. National Museum (U.S.N.M. No. 2680).

Type Host: *Rattus norvegicus* (Erxleben).

Type Locality: Florida, Tampa, 15 October, 1947, R. L. Scott.

Remarks: This species is named in honor of Dr. George R. Larue, Professor Emeritus of Zoology, University of Michigan.

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Report of the Brayton H. Ransom Memorial Trust Fund

Funds On Hand, Jan. 1, 1959	$1877.38
Receipts: Interest rec'd in 1959	60.92
Contribution to principal	5.00
Disbursements: Grant to Helminthological Society of Washington	10.00
Balance On Hand, Dec. 31, 1959	1933.30

A. O. Foster
Secretary-Treasurer

MINUTES

Three Hundred Sixty-Fifth through the Three Hundred Seventy-Second Meetings

365th meeting: Sternberg Auditorium, Walter Reed Army Institute of Medical Research, Washington, D. C., 21 October 1959. President Stirewalt appointed D. A. Shorb, D. R. Lincicome, and C. M. Hermann a Steering Committee to consider means of commemorating the 50th anniversary of the Society's founding. Papers presented: Diagnostic and clinical investigations on human paragonimiasis in South Korea, by Sadun; Thermal factors in the development of *Dirofilaria uniformis* in *Anopheles quadrimaculatus,* by Duxberry; Susceptibility of wild animals to *Schistomsoma mansoni* infection, by Bruce; Effects of ionizing radiations on *Plasmodium gallinaceum* by Ward.

366th meeting: Home Economics Building, Agricultural Research Center, Beltsville, Maryland, 20 November 1959. Dr. G. R. LaRue elected honorary member. Recording Secretary approved as representative of this Society on the Council of American Society of Parasitologists. The following slate of officers was elected: G. W. Luttermoser, President; C. G. Durbin, Vice-President; L. A. Jachowski, Recording Secretary; E. M. Buhrer, Corresponding Secretary and Treasurer; G. F. Otto, Editor of the Proceedings. Chairmen were appointed to the following Golden Anniversary subcommittees: C. G. Huff—Program; L. J. Oliver—Banquet; G. F. Otto—Publications. Papers presented: Diagnosis of stephanuriasis by a micro double-diffusion agar precipitin technique, by Tromba and Baisdan; Preliminary report of the effectiveness of three management systems for control of internal parasites of lambs by Kates, Turner, Lindahl, Whitmore and Enzie; An experiment on combined active and passive immunization against the cattle lungworm, *Dictyocaulus viviparus,* by Vergers and Tucker; Cultivation of *Ascaris* tissues, by Doran; and moving pictures of the feeding habits of reduviids, by Lavoirpierre.

367th meeting: Biology Building, Howard University, Washington, D. C., 1 December 1959. Joint meeting with the Washington Tropical Medicine Association and the Howard University Chapter of the Society of the Sigma Xi to hear Sir Philip Manson-Bahr speak on wild game and man in Central Africa.

368th meeting: McMahon Hall, Catholic University, Washington, D. C., 20 January 1960. Plans for 50th Anniversary Celebration approved to include a tripartite program on *8 October 1960* under the title of Perspectives in parasitism. Two scientific sessions will be held during the day at the

University of Maryland, College Park, Maryland, and a banquet will be held in the evening. Three invited speakers will address the morning session. Informal discussion groups will organize in the afternoon. Papers presented: Discovery of Trichinae and determination of their history and pathogenicity by Schwartz; Serologic diagnosis of *Schistosoma mansoni infections,* by Anderson.

369th meeting: Francis Scott Key Hall, University of Maryland, College Park, Maryland, 17 February 1960. Annual report of Treasurer approved. The American Society of Parasitologists accepted the Society as an affiliate. Papers presented: Salivary ducts of trombiculid mites, by Wharton and Yunker; Survival of *Rickettsia mooseri* in *Echnidolaelaps echnidninus,* by Grayson; An inexpensive microbalance, by Kanunga; Respiratory quotient and fat depletion in *Strongyloides papilliosus* infective larvae, by Costello; and observations on the rat nematode, *Nippostrongylus brasiliensis,* by Haley.

370th meeting: Wilson Hall, National Institutes of Health, Bethesda, Maryland, 25 March 1960. Papers presented: Etiology and treatment of chronic respiratory disease of rats, by McPherson; Three common parasites of rhesus monkeys, by Kinard; Four internal parasites of rhesus monkeys by Allen; and Observations on calcareous corpuscles of cestodes, by von Brand. An exhibit on Bartonellosis in man and animals was prepared, by Habermann and Faulkner.

371st meeting: School of Hygiene and Public Health, Johns Hopkins University, Baltimore, Maryland, 15 April 1960. Papers presented: Agar diffusion studies in trichinosis by, Van Peenen; Salivary chromosomes of populations of *Anopheles albimanus,* by Hobbs; Population density effects on developmental dynamics of *Hymenolepis diminuta* in the albino rat, by Roberts; and Parasitological reconnaissances in some newly independent tropical countries, by Schiller, McGowan, Bang and Rozeboom.

372nd meeting: Annual picnic at Log Lodge, Agriculture Research Center, Beltsville, Maryland, 21 May 1960.

The following were elected to membership at the meetings indicated: 365th—M. T. Hutchinson, S. Lefkowitz, R. N. Rossan, R. R. Brenes, M., R. Desrochers, C. E. Dieter, D. J. Zinn, B. M. Zuckerman, K. C. Sanwal, L. G. Warren, R. F. Hutton, E. M. Noffsinger, F. B. Strubble; 366—C. R. Schneider, W. J. Zimmerman, W. E. Blaylock; 368th—M. J. Ulmer, T. M. Schwink, I. Singer; 369th—D. G. Murphy, N. T. Briggs, D. A. Slack, H. Engelbrecht; 370th—J. C. Parker, L. A. Salas, F., S. P. Gupta, P. Nasir, H. L. Rhoades, R. F. Myers; 371st—B. Bezubik; 372nd—W. Ashley, Jr., L. E. Peters, Jr., Mary Beverley-Burton.

LEO JACHOWSKI, JR.
Recording Secretary

IN MEMORIAM

Maurice Blood Linford

March 9, 1901—September 24, 1960

Professor of Plant Pathology, University of Illinois
Member Helminthological Society of Washington Since March 1937

Paul Bartsch

August 14, 1871—April 24, 1960

Paul Bartsch died on April 24, 1960 in his home located on a 450-acre estate overlooking the Potomac, near Lorton, Virginia. There he lived and continued to work following his retirement from the Smithsonian Institution. The estate, converted by him into a wildlife refuge, was visited by hundreds of youngsters and adults year after year. He lived in the midst of the plant and animal life that he loved and knew intimately, as only a naturalist of his stature and familiarity with the outdoors could know.

Dr. Bartsch was born in Silesia, Germany, August 24, 1871, and came to the United States with his parents when he was 11 years old. He was educated in the University of Iowa where he received the B.S. (1896), M.S. (1899), and Ph.D. (1905) degrees. His professional career was begun in the Smithsonian Institution in 1896 as an aide in the Division of Mollusks of the U. S. National Museum. There he worked until his retirement in 1946, and there he continued his professional association as a Research Associate until his death. At the time of his retirement, the Division of Mollusks, which he headed for many years, was concerned not only with mollusks, but also with cenozoic invertebrates, helminths, and corals.

Although Paul Bartsch was best known as a malacologist who had written many technical taxonomic and morphological monographs on various groups of mollusks, he was far from being only a conventional systematist. It is true that he was the author of species and genera and a student of their

relationships in family and higher systematic groups. He was, however, first and foremost a naturalist, and evinced even early in life more than an aptitude—actually, a passion for observing and studying living things, plants as well as animals, in their natural habitat, collecting and preserving them, and, finally, determining their morphologic and systematic status. All of nature's creatures were of interest to him, mammals, birds, insects and other invertebrates, ferns, flowering plants, in fact, any living thing that his keen eyes could detect. In the course of fully half a century of professional activity he led, or participated in, numerous biological expeditions, collecting on land and in the sea, not only in this hemisphere but also in other parts of the world. His most extensive expedition, lasting two years, was on the steamer Albatross. This afforded him an opportunity to make extensive collections, fresh-water, marine, and terrestrial, in the Philippines. These and the many other collections he made have enriched the biological material available in the U. S. National Museum to such an extent that it would take a sizable group of specialists to work over the as yet unstudied specimens that he deposited there.

This is not the place to evaluate Dr. Bartsch's contributions to malacology, but rather to point out his interest in parasitology and his association with the Helminthological Society of Washington. His interest in parasitology stemmed in part from his studies of medical malacology, in part from his association in Washington with such eminent parasitologists as Stiles, Cobb, Ransom, Hall, and others, but in the main from his contacts for about 20 years with graduate students in the George Washington University. As Professor of Zoology, he had close association with rather mature graduate students, nearly all of whom already had established, in the various governmental laboratories where they were employed, a record of capacity for independent research. Among them, at least 10 obtained their doctorates and several others the master's degree in parasitology. Because of these contacts Dr. Bartsch joined the American Society of Tropical Medicine, the American Society of Parasitologists, and the Helminthological Society of Washington. His membership in the last-named Society apparently dates back to 1920. Evidence of his interest in the Society is shown by the fact that he served as its President in 1929-1930, represented it on the Council of the Washington Academy of Science 1930-1931, and was elected a Life Member on December 21, 1937. Many members of the Society will recall the several meetings held in his home in Washington, D. C., and at least one Spring meeting on his estate in Virginia.

The writer of this sketch knew Dr. Bartsch for more than 40 years. He was of tall stature, slender, striking in appearance, and perfectly at ease in any company. His broad training in the natural sciences, his wide and varied experience as a biological explorer in many parts of the world, his contacts with graduate students of diverse biological interests, and his participation in various social and civic affairs in his community made him a well-rounded human being rather than one with only the specialized knowledge that is understood by the few. His passing further diminishes the number of biologists of the "old school" whose curiosity and interests were too extensive to limit themselves to only a very small segment of the science that treats of the self-renewing drama that constitutes life.

BENJAMIN SCHWARTZ

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